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# SOME CONTINGENCIES IN THE EFFECTS OF THE SOURCE OF A COMMUNICATION ON THE EVALUATION OF THAT COMMUNICATION[1]

ROBERT S. WYER, JR., AND SANDRA SCHWARTZ

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Subjects were asked to evaluate one of four types of communications—statements about Negroes, statements about social issues, cartoons, or photographs. The evaluations were investigated as a function of the quality of the communications that preceded them, whether the communications in the series originated from one source or from different sources, the logical consistency of the communications in the series, and incentive to perform well on the rating task. Results generally support the hypothesis that source effects on communication evaluations are predictable from congruity theory only to the extent that the communication is contradictory to previous communications from the source that form the basis for the evaluation of that source. When the degree of contradiction is low, the tendency to resolve incongruities may be offset by a more general tendency to evaluate communications in relation to a comparison level, the value of which increases with the quality of the communications previously experienced. The effect of this latter tendency is opposite to that predicted by congruity theory. The influence of shift in comparison level appears to increase with incentive to perform well when all messages are attributed to the same source, but to decrease with incentive when the communications ostensibly come from different sources. Implications of results for congruity theory (Osgood & Tannenbaum, 1955) and exchange theory (Thibaut & Kelley, 1959) are discussed.

The research reported in the present paper investigated the implications of two theoretical formulations, congruity theory (Osgood & Tannenbaum, 1955) and exchange theory (Thibaut & Kelley, 1959), for predicting the manner in which evaluations of a communication are affected by its source. Congruity theory predicts that when two objects are positively associated, the evaluation of one will be displaced toward the evaluation of the second. Therefore, a communication from a positively regarded source should be evaluated more positively than the same communication if attributed to a negatively regarded source. Exchange theory, on the other hand, assumes that objects are evaluated in relation to a comparison level (CL), or point of affective neutrality along a continuum of ordered stimuli.[2] A person's CL is assumed to increase with the quality of the stimuli he expects to evaluate. If the personal source of a communication, who may often be evaluated on the basis of communications from him in the past, determines this expectancy, the formulation predicts that a communication from a positively regarded source would be evaluated *less* positively than the same communication if attributed to a negatively regarded source.

While predictions based upon the two theoretical positions previously described appear contradictory, it should be noted that the processes underlying the resolution of incongruities, on one hand, and shift in CL, on the other, may operate simultaneously. Hence, it may be more useful for present purposes to consider what factors affect the relative dominance of the processes than to attempt to test the relative validity of the theoretical formulations pertaining to them.

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[1] This research was supported by National Science Foundation Grant GS 1356. Thanks are extended to Richard Bednar for his assistance with various aspects of the study.

[2] Thibaut and Kelley (1959) restrict their analysis to judgments of the quality of behavioral outcomes along an affective continuum. However, their approach can be easily generalized to judgments of stimuli in any domain. Their formulation makes similar predictions to an adaption-level theory of judgment (Helson, 1964), but avoids several of the assumptions underlying the latter theory.

Consider a situation in which subjects are asked to evaluate a series of communications and are led to believe either that all communications are from the same source or that each communication is from a different source. The quality of the communications a subject experiences may affect his expectancy for future communications and, therefore, his CL, regardless of whether the communications emanate from one source or from several sources. When each communication is attributed to a different person, no incongruity between the quality of any communication and the quality of its source should arise, and therefore exchange-theory predictions of a negative relationship between evaluations of a particular message and the quality of the messages preceding it may be supported. However, when all communications are attributed to a single source, an incongruity between any given communication and those preceding it is more apt to occur, and the effects of resolving this incongruity may often be sufficiently strong to override the more general effect of shift in CL.

It is unclear, however, whether the resolution of incongruities between source evaluations and communication ratings results from an attempt to reduce purely affective differences between the stimuli involved, or whether it occurs only when the communication being rated is contradictory in meaning to previous communications from the source that provide the basis for his evaluation. (The assumption that the reduction of attitudinal inconsistencies is the result of applying logical or "psycho-logical" rules of inference to verbal statements pertaining to these attitudes is not new; cf. McGuire, 1960; Abelson & Rosenberg, 1958.) Frequently, a communication can be given a variety of interpretations. If the communication is attributed to a particular person, a subject may interpret it in a way that seems most probable on the basis of previous information about characteristics of that person. The evaluation of the communication may be affected as a result.

It is not, of course, always the case that if evaluations of a communication and its source differ, they are contradictory. For example, a comedian who is believed to be of high quality because the jokes he tells are generally very funny may, nevertheless, be deemed capable of occasionally telling a poor joke. The jokes told by the comedian, while they differ in their affective quality, are not logically inconsistent with one another. In such a case, a tendency to displace the evaluation of the communication toward the evaluation of the source may be less likely to exist. In fact, if the quality of the jokes expected from the comedian is high, any given joke told by him may be evaluated less positively than if it had been told by a relatively poor comedian, as predicted on the basis of a shift in CL.

In the present study, subjects were exposed to one of four series of communications and told either that all messages in the series came from the same person or that each came from a different person. Messages in one series (opinion statements differing in their favorableness toward Negroes) were assumed to be highly contradictory if made by a single person; messages in a second series (opinion statements about social issues which differed along a continuum of liberalism–conservatism) were assumed to be moderately contradictory; and messages in the other two series (cartoons and photographs of varying quality) were assumed to be minimally contradictory. The following specific hypotheses were made:

1. When communications are attributed to different sources, the evaluation of a message is related negatively to the quality of the messages preceding it in the series for all four types of communications.
2. When each communication is attributed to the same source, the evaluation of any given message is related positively to the quality of the communications preceding it, but *only* to the extent that it is contradictory in meaning to the previous communications. Specifically, the relationship is most positive in the domain of statements about Negroes and least positive in the domains of cartoons and photographs.

In addition to testing the previous hypotheses, the effects of two supplementary variables were investigated. In the domains of statements about Negroes and statements about social issues, an attempt was made to manipulate directly the degree of logical consistency among statements involved. In one condition, statements that were diametrically opposite to one another in meaning were included in the series, and in another condition somewhat less contradictory statements were presented. Second, in the domains of cartoons and photographs, the effects of incentive to perform well were determined. While specific predictions concerning the effects of incentive were not made, consideration of the variable seemed important in light of evidence (e.g., Brown, 1953) that high involvement may increase the tendency to maintain an original position despite variation in stimuli relevant to that position. The tendency could lead to apparent support for congruity theory under high-involvement conditions even when the stimuli presented are not contradictory in meaning. On the other hand, it seems equally reasonable to expect that an incentive to perform well would increase attentiveness to the stimuli being presented, which could increase the tendency to use previous communications as referents for evaluating subsequent ones and, therefore, increase the effect of the previous stimuli on CL. The alternative possibilities seem worth exploring.

## METHOD

### *Overview*

Introductory psychology students were asked to rate a series of 10 communications. The messages were of one of the four types described previously. Five of the communications in each domain were positive (i.e., they had high normative values along an evaluative dimension), and five were negative. The order of presentation was varied so that evaluation of a particular test communication could be investigated as a function of its context (defined in terms of the quality of the communications preceding it). Under one set of instructional conditions, all communications were attributed to a single person; under a second set of conditions, each message was attributed to a different source.

In each of two stimulus domains, statements about Negroes and statements about social issues, the logical consistency of the positive and negative statements contained in the series was varied. In each of the two other domains, cartoons and photographs, the incentive to make accurate judgments was manipulated.

A total of 384 subjects participated, 16 under each experimental condition in which communications were attributed to a single source, and 8 under each condition in which each message allegedly came from a different source.

### *Selection of Stimuli*

*Statements about Negroes.* Normative data on the favorableness of a series of 30 statements about Negroes, selected from the item pool developed by Hinckley (1932), were obtained from 80 college students by asking them to rate each statement along a scale from −10 (very unfavorable to Negroes) to 10 (very favorable to Negroes). Ten additional statements that were assumed to be diametrically opposite in meaning to 10 statements in the original set were also rated. On the basis of these data, five positive (favorable to Negroes) statements and five negative (unfavorable to Negroes) statements were selected. Selections were made on the basis of normative data, without regard to content. The five positive statements had normative values of 2.3, 1.9, 2.3, 2.1, and 2.0; the five negative statements had values of −3.1, −2.9, −2.2, −2.1, and −2.6. The first and last statements in the positive set ($P_1$ and $P_2$, respectively) and the first and last statements in the negative set ($N_1$ and $N_2$, respectively) were used as test stimuli. They were:

$P_1$: A Negro with a little education is more productive than a white man.

$P_2$: Behavior according to white standards should not be expected from the Negro without granting him the same position in society as the white man.

$N_1$: A Negro can be recognized as a human being without raising him to the level of the whites.

$N_2$: Many times I find the Negro defiant and rude.

To construct a second series of test stimuli that was more clearly, logically inconsistent, statements $P_1$ and $N_1$ were replaced with the following two statements:

$P_{1a}$: In all my dealings with the Negro he has been agreeable and courteous.

$N_{1a}$: The black and white races can never merge, nor will the Negro ever reach the social equality of the white man.

The normative values of these statements, which were assumed to be contradictory to $N_2$ and $P_2$, were 2.2 and −2.7, respectively.

*Statements about social issues.* Forty items were constructed that pertained to a variety of social and political issues, for example, parental disciplinary practices, organized religion, sexual permissiveness, Communism, the Vietnamese war, etc. The statements were expected to differ in the degree to which they reflected a conservative or liberal point of view. Eighty college students were asked to rate each statement along a scale from −10 (extremely conservative) to 10 (extremely liberal). They were also asked to indicate whether they personally agreed or disagreed with each statement. Five positive (liberal) statements, with mean values of 2.1, 1.9, 2.7, 3.0, and 2.1, and five negative (conservative) statements, with mean values of −2.1, −1.5, −2.7, −3.1, and −2.4, were selected. To insure that these statements were not logically inconsistent, agreement with positive items was correlated with agreement with each negative item. The average of these correlations was −.04.

The first and last statements in each set of five were selected as test stimuli. They were:

$P_1$: Most censorship of books and movies is a violation of free speech and should be abolished.

$P_2$: Complete sex-education courses should be given to all high school students.

$N_1$: The facts on crime suggest that we will have to crack down on some people if we are going to save our moral standards.

$N_2$: There would be no juvenile delinquents if parents would just insist on strict obedience from their children.

Other statements in the series of 10 pertained to such issues as divorce, trade with Russia, institutionalized religion, etc.

To obtain a second series of test stimuli that were logically inconsistent, two statements assumed to be contradictory to $N_2$ and $P_2$ were constructed. These statements, which replaced $P_1$ and $N_1$ in the second series of stimuli presented, were:

$P_{1a}$: If parents were less authoritarian and gave their children more freedom, there would be fewer juvenile delinquents.

$N_{1a}$: Sex education is a family matter and should be left to the parents.

No normative data were collected on the statements.

*Cartoons.* A series of 50 cartoons from various magazines were presented to 80 college students with instructions to rate them along a scale from 0 (not at all funny) to 20 (very funny). On the basis of the data, five positive (funny) cartoons, with mean values of 13.6, 14.7, 15.3, 15.2, and 15.2, and five negative (unfunny) cartoons, with mean values of 4.9, 4.8, 5.9, 5.8, and 6.1, were selected. The fifth positive cartoon (P) and the first negative cartoon (N) were used as test stimuli.

To insure the credibility of instructions that each cartoon rated was drawn by a single source, all 10 cartoons were redrawn by a student artist using a common style.

*Photographs.* Forty photographs of persons were selected from various magazines and collections and were reproduced on slides. Eighty undergraduate students were shown the slides and asked to rate each picture along a scale from 0 (not at all interesting) to 10 (very interesting). On the basis of the data, five positive (interesting) photographs, with mean values of 6.2, 6.2, 6.1, 6.8, and 6.2, and five negative (uninteresting) photographs, with mean values of 3.2, 2.8, 3.0, 3.7, and 3.3, were selected. The fifth positive photograph (P) and the first negative photograph (N) were used as test stimuli.

## *Presentation of Materials and Instruction*

*Statements about Negroes and social issues.* The manner of presenting stimuli in the two domains and the instructions given to subjects were similar. Statements were presented in booklets. Each statement was presented on a different page, along with a 21-point scale to use in evaluating it. Statements were ordered such that either the block of five positive statements preceded the five negative statements, or the five negative statements preceded the five positive ones. Within each of these conditions, the position of the two test stimuli at each level was counterbalanced. Four orders of presentation were, therefore, used. In one case, the first, fifth, sixth, and tenth statements were: $P_1$, $P_2$, $N_1$, and $N_2$; in a second, the statements were $P_2$, $P_1$, $N_2$, and $N_1$; in a third, $N_2$, $N_1$, $P_2$, and $P_1$; and in the fourth, $N_1$, $N_2$, $P_1$, and $P_2$. In the condition in which maximally inconsistent test stimuli were presented, $P_{1a}$ and $N_{1a}$ replaced $P_1$ and $N_1$ in each of the aforementioned series. Therefore, under the maximally inconsistent condition the sixth statement was always presented immediately following a statement with which it was highly contradictory.

Under conditions in which statements were attributed to a single source, subjects were told that the study was designed to determine how attitudes toward a person change with increasing amounts of information about him. They were told that each statement they would see was made by the same person during the course of an informal conversation and that they should evaluate each statement as it was presented along the scale below it. The scales ranged from −10 (very unfavorable to Negroes) to 10 (very favorable to Negroes) in one of the domains considered, and from −10 (very conservative) to 10 (very liberal) in the other. At seven points during the presentation of the statements, subjects were also asked to rate the source of the communications along a dimension identical to that used in rating the statements themselves. Source ratings were made before any statements from the source were presented; after the first, fourth, and fifth positive statements; and after the first, fourth, and fifth negative statements (i.e., before the first statement, and after Statements 1, 4, 5, 6, 9, and 10).

When statements were attributed to different sources, subjects were simply told that each statement they would see was made by a different person on the basis of an informal interview and were told to rate each statement along the scale provided. Source evaluations were not made under such conditions.

*Photographs and cartoons.* Stimuli in the two domains were presented on slides in one of two orders. In one condition, the five positive stimuli preceded the five negative stimuli; within this sequence, stimulus P was presented fifth, and N was presented sixth. In the second condition, the order of presentation was exactly reversed.

The experimenter introduced the experiments to subjects by noting that one of the largest areas of interest in the field of psychology is advertising and told them that the study was concerned with determining what elements in an advertisement are appealing. When cartoons were to be presented, subjects were told that the experiment was specifically concerned with determining what sorts of humor are appealing so that this information could be applied to the design of advertisements that would be of interest to the public. Instructions then paralleled those used in rating statements about Negroes and social issues. That is, subjects exposed to photographs under one condition were told that they would be presented a series of portraits that were all taken by the same commercial photographer. They were instructed to estimate the interest value of each photograph (how interesting it was to them) along a scale from 0 (not at all interesting) to 20 (very interesting). They were also asked to record their opinion of the photographer "based upon his ability to capture interest in a photograph" at the seven points in the series indicated previously. These ratings were made along a scale from 0 (no ability) to 20 (great ability). Subjects under a second condition were told that each photograph they would see was taken by a different photographer and were asked only to rate each photograph as it was presented, but not to evaluate the source.

Subjects exposed to cartoons were given analogous instructions. They were told that the cartoons they would see had been drawn either by the same cartoonist or by different cartoonists and that the cartoons had been redrawn in order to conceal his or their identity. They were asked to rate each cartoon along a scale from 0 (not at all funny) to 20 (extremely funny) and, when appropriate, to rate the source along a scale from 0 (no ability) to 20 (great ability).

*Manipulation of incentive.* Half of the subjects asked to rate either photographs or cartoons were given an incentive to perform well by telling them that the study was sponsored by "Commercial Data Corporation, a Chicago advertising firm," which would be performing subsequent research in this area and which would select a representative group of students to participate for pay if the subjects were interested and if their performance on the experimental task warranted. The salary to be offered was given as \$3–\$5 per hour. For the remaining subjects, the aforementioned instructions were omitted.

Subjects who were given an incentive to perform well were asked to record on the first page of their answer sheet whether they would like to participate in other studies if they were chosen. With one exception, all subjects asked to rate photographs under such conditions reported that they would. Nine of 48 subjects asked to make ratings of cartoons indicated that they would not like to participate in subsequent studies. The nine subjects were eliminated from the experiment. Three additional subjects, selected randomly, were eliminated from analyses to obtain proportional cell frequencies at each combination of experimental conditions. (This left 12 subjects under each condition in which cartoons were attributed to a single source and 6 under conditions in which they allegedly came from different sources.)

## RESULTS

Preliminary to testing the specific hypotheses of the present study it was necessary to determine the validity of the assumption that source evaluations would increase over series of positive messages and would decrease over series of negative ones. Therefore, an analysis of variance was performed on source evaluations in each communication domain as a function of order of presentation, point in the series of communications at which the evaluations were made, and either the degree of logical consistency of the test messages presented (in the case of statements about Negroes or about social issues) or incentive to perform well (in the case of cartoons or photographs). Data relevant to the interaction of order of presentation and serial position, which was highly significant in each analysis, are shown in Table 1. Comparisons of source evaluations after the fifth communi-

TABLE 1

SOURCE EVALUATIONS IN EACH DOMAIN AS A FUNCTION OF ORDER OF PRESENTATION AND MESSAGE AFTER WHICH THE EVALUATIONS WERE MADE

| Presentation order (A) in each content domain | Number of message after which the evaluation was made (B) | | | | | | | | *F* ratio | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | 0[a] | 1 | 4 | 5 | 6 | 9 | 10 | *M* | A | B | A × B |
| Statements about Negroes | | | | | | | | | 10.34* | 9.72* | 90.26** |
| Positive–negative | −1.94 | 3.44 | 6.25 | 5.65 | .31 | −3.22 | −4.88 | .80 | | | |
| Negative–positive | −1.40 | −4.84 | −6.12 | −6.16 | 1.65 | 4.65 | 5.16 | −1.00 | | | |
| Statements about social issues | | | | | | | | | 8.68* | .45 | 49.58* |
| Positive–negative | .13 | 4.38 | 5.38 | 4.88 | −1.06 | −1.97 | −2.91 | 1.26 | | | |
| Negative-positive | .38 | −3.69 | −3.59 | −4.12 | 1.97 | 3.34 | 2.66 | −.44 | | | |
| Cartoons | | | | | | | | | 9.00* | 8.73* | 16.31* |
| Positive–negative | 13.36 | 12.04 | 13.32 | 12.36 | 9.89 | 9.82 | 10.18 | 11.57 | | | |
| Negative–positive | 12.64 | 5.75 | 7.89 | 6.93 | 11.54 | 10.92 | 10.57 | 9.47 | | | |
| Photographs | | | | | | | | | 1.14 | 9.72* | 19.84* |
| Positive–negative | 14.50 | 12.62 | 14.32 | 13.06 | 8.68 | 11.06 | 11.36 | 12.22 | | | |
| Negative–Positive | 14.68 | 10.44 | 11.50 | 9.50 | 13.68 | 15.74 | 14.38 | 12.84 | | | |

[a] Initial impression.

* $p < .05$.

** $p < .01$.

cation in each series clearly validate the assumption in question.

Two unexpected contingencies in results not shown in Table 1 may be worth mentioning. First, when the communications were about Negroes, the interaction of presentation order and serial position was significantly contingent upon the degree of logical consistency of the messages presented ($F = 3.99$, $df = 6/336$, $p < .01$). Specifically, when messages were maximally contradictory, evaluations of the source following communications occurring near the end of the series were less extreme. Second, when the communications involved were cartoons, source ratings were more favorable under high-incentive conditions than under low-incentive conditions ($F = 4.77$, $df = 1/48$, $p < .05$). However, since neither of these effects was replicated in the other domains in which either logical consistency or incentive was manipulated, their reliability is questionable.

### *Evaluation of Communications*

Ratings of each type of communication were analyzed separately. Only ratings of the fifth and sixth communications in each series were considered; such communications occurred either immediately after a set of positive communications (in a positive context) or immediately after a set of negative communications (in a negative context).

It was predicted that if communications were attributed to different sources, they would be rated lower when presented following positive communications than when presented following negative ones. However, if messages were ascribed to a single source, the effect of context was expected to depend upon the content domain, being most positive in the domain of statements about Negroes and least positive in the domains of cartoons and photographs. Data relevant to these hypotheses are presented in Table 2.

To assist in testing the aforementioned hypotheses and to provide information concerning effects of the other experimental variables considered, separate analyses of variance were performed on evaluations made in each domain. Evaluations of statements about Negroes and statements about social issues were each analyzed as a function of type of message (positive vs. negative), context (positive vs. negative), source conditions (single source vs. different sources), and logical consistency. Analyses of cartoon ratings and photograph ratings were similar except that the logical consistency variable was replaced by the incentive variable.

TABLE 2

EVALUATIONS OF COMMUNICATIONS IN EACH CONTENT DOMAIN AS A FUNCTION OF CONTEXT AND SOURCE CONDITIONS

| Content domain | Different sources | | Single source | |
|---|---|---|---|---|
| | Positive context | Negative context | Positive context | Negative context |
| Statements about Negroes | .63 | −1.75 | 1.80 | .03 |
| Statements about social issues | −.25 | 1.28 | .47 | .16 |
| Cartoons | 6.79 | 10.32 | 9.20 | 9.54 |
| Photographs | 6.56 | 11.56 | 8.44 | 12.16 |

Two effects were of particular relevance to the major hypotheses under consideration. One, the main effect of context, was significant in both the domain of statements about Negroes ($F = 12.31$, $df = 1/88$, $p < .001$, $MS_e = 15.12$) and the domain of photographs ($F = 49.62$, $df = 1/88$, $p < .001$, $MS_e = 16.64$), approached significance in the domain of cartoons ($F = 3.81$, $df = 1/76$, $p < .10$, $MS_e = 21.71$), but did not reach significance in the domain of statements about social issues ($F < 1.00$; $MS_e = 15.74$). The second, the interaction of context and source conditions, reached significance only in the domain of cartoons ($F = 4.39$, $df = 1/76$, $p < .05$).

Data in Table 2 indicate that in each domain except that of statements about Negroes evaluations of communications attributed to different sources were lower when presented following positive messages than when presented following negative ones.

The results are consistent with the assumption that CL is affected by the initial communications in the series. However, when communications about Negroes were attributed to different sources, they unexpectedly received higher ratings when presented in a positive context than when presented in a negative context; the magnitude of the effect did not differ from that obtained when all statements were attributed to the same source. The implications of this finding will be discussed presently.

When communications were attributed to the same source, those which were presented in the context of positive messages were rated as significantly higher in the domain of statements about Negroes, slightly higher in the domain of statements about social issues, slightly lower in the domain of cartoons, and significantly lower in the domain of photographs than were the same communications when presented in the context of negative messages. Except for the nonsignificant relatainship obtained in the domain of cartoons (which was significantly less negative than when the cartoons were attributed to different sources), the data are consistent with the general hypothesis that a displacement of message evaluations toward the evaluations of their source occurs only to the extent that the communications involved are contradictory in meaning to previous communications from the source.

### *Supplementary Findings*

*Effect of direct manipulation of logical consistency.* A more direct test of the hypothesis that support for congruity theory depends upon the degree of logical inconsistency of the communications ascribed to the source was not supported. In neither the domain of statements about Negroes nor the domain of statements about social issues was the effect of context favorableness more pronounced when test communications were diametrically opposed to the statements immediately preceding them.

*Effect of incentive to perform well.* A significant triple interaction of context, incentive, and number of sources occurred in analyses of ratings of both cartoons ($F = 11.40$, $df = 1/76$, $p < .001$) and photographs ($F = 4.11$, $df = 1/88$, $p < .05$). Data relevant to the interactions, which are presented in Table 3, indicate that in both domains the introduction of an incentive to perform well decreased the magnitude of the negative relationship between communication evaluations and the quality of communications preceding them when each communication ostensibly came from a different source, but increased the magnitude of the relationship when communications were all attributed to the same source. These data do not support the hypothesis that arousal generated by providing an incentive to perform well decreases the subject's sensitivity to change in the quality of stimuli present. Rather, if differences in the effects of context in the two domains are due primarily to shifts in CL, the results suggest that incentive increases the tendency to use preceding stimuli as referents for evaluating subsequent ones (and therefore increases the effect of CL) when the stimuli originate from a single source, but decreases the tendency to use previous stimuli as standards for comparison when the stimuli are attributed to different sources.

TABLE 3

EVALUATIONS OF CARTOONS AND PHOTOGRAPHS AS A FUNCTION OF INCENTIVE, CONTEXT, AND SOURCE CONDITIONS

| Content domain | Different sources |  | Single source |  |
|---|---|---|---|---|
|  | Positive context | Negative context | Positive context | Negative context |
| Cartoons |  |  |  |  |
| High incentive | 9.59 | 9.83 | 8.12 | 11.12 |
| Low incentive | 4.69 | 10.69 | 10.00 | 8.34 |
| Photographs |  |  |  |  |
| High incentive | 7.00 | 10.00 | 7.88 | 12.12 |
| Low incentive | 6.12 | 13.12 | 9.00 | 12.18 |

Various other interactions involving the incentive variable reached significance. However, since the results were not consistent over domains and since their interpretation is unclear, they are not reported.

## DISCUSSION

The results of the present study clearly indicate that a difference in affective quality between a communication and its source is not a sufficient condition for resolution of incongruities between the evaluations of the objects; in fact, under certain conditions, the quality of the source may have exactly opposite effects to those predicted by congruity theory.

It was speculated that two processes underlie judgments of stimuli originating from a source. First, previous communications, either from one source or from different sources, may alter a subject's expectancy for the type of communications he will receive in the future and, therefore, may change his CL for evaluating subsequent communications. Second, when communications originate from a single source, subjects may tend to interpret communications in a manner that is least contradictory to previous communications from the source. Under conditions in which inconsistencies in meaning are confounded with differences in affective quality, the latter process may lead to a positive relationship between evaluations of a communication and the quality of previous communications from the source that may often be sufficiently strong to offset the negative relationship between the variables created by shift in CL. When there is little contradiction among the communications involved despite differences in their affective quality, attempts to resolve incongruities may not occur, and the effect of shift in CL would thus be more apparent.

Results obtained in different stimulus domains are generally consistent with the previous formulation; however, two questions arise. First, although in three of the four cases in which communications were attributed to different sources they were rated as less positive following positive communications than following negative ones, the reverse relationship occurred in judgments of statements about Negroes. The latter relationship suggests that it is not necessary for a series of communications to originate from a single person for subjects to tend to interpret them in a manner that is consistent with those presented earlier in the series. Possibly the juxtaposition of a set of statements conveying a similar attitude is sufficient to affect the manner in which subsequent messages are interpreted; the source of the statements may be relatively irrelevant. One implication of such a conclusion is that the resolution of incongruities is not due to motivating properties of the inconsistencies, but instead is a consequence of a cognitive "set" to interpret statements in a manner similar to that of other statements in the context of which they are embedded.

Second, the negative relationship between ratings of cartoons and the quality of the cartoons preceding them was significantly less pronounced when the cartoons were attributed to a single source than when they originated from different sources. Since cartoons and photographs were expected to be equally independent communications, the difference in context effects in the two domains was somewhat unexpected. To be consistent with the hypothesis raised in this study, one would need to argue that poor cartoons from a good cartoonist or good cartoons from a poor cartoonist are regarded as somewhat contradictory, while good photographs taken by a poor photographer or poor photographs by a good photographer are not. Unfortunately, direct evidence to support the interpretation is not presently available.

The failure for the direct manipulation of logical consistency to affect the magnitude of source effects on communication ratings also raises some question about the adequacy of the assumption that the tendency to resolve incongruities increases directly with the degree of contradiction between the statements involved. Since the effectiveness of the consistency manipulation was not validated directly, it is possible that the conditions did not differ substantially in the degree to which the statements involved were contradictory. However, the effect of the variable on source evaluations when statements about Negroes were presented suggests that the manipulation was effective in at least one domain. It may be equally reasonable to speculate that if statements are diametrically opposed, an incredulity factor may be operative that decreases the predicted effect. A more systematic investigation of this issue is required.

A contingency in the tendency to use previous stimuli as standards for comparison, and therefore in the effect of these stimuli on CL, was indicated by the interaction of context and source conditions with the incentive to perform well. The contingency could be accounted for by a tendency for incentive to increase subjects' motivation to comply with the implicit expectancies of the experimenter. That is, those subjects who are told that communications come from a single source may infer that they are expected to use previous communications from this source as standards, while those who are told that the stimuli being judged come from different sources may infer that they are expected to evaluate the stimuli independently. Attempts to comply with the expectancies may increase when an incentive is provided. If such an interpretation is correct, the effect of CL in the present study is clearly not a perceptual phenomenon, but rather is one which depends upon the subject's interpretation of the most appropriate procedure for responding in the particular situation in which he finds himself.

Finally, certain relationships suggested by analyses of source evaluations (Table 1) may be worth mentioning. Final source evaluations (after the tenth communication) were higher when positive communications were presented last than when they were presented first, indicating a recency effect. However, the mean evaluation of the source over trials was generally greater when positive communications were presented first. The latter finding seems to indicate that communications had a greater effect upon source ratings when presented earlier in the series, a conclusion that might be interpreted as support for a primacy effect. The latter effect is very likely an artifact. Since the effects of initial communications enter into all source evaluations in the series being averaged, while the effects of subsequent messages enter only into later source evaluations, the earlier messages necessarily exert a disproportional influence on the average evaluation. This would be true even if there were neither a primacy nor a recency effect on the ratings based upon any individual communication. The methodological problem is worth noting.

The only domain in which the mean evaluation of the source was *not* greater when positive messages were presented first was that of photographs. The domain was also the only one in which a strong contrast effect was apparent. The results in combination suggest an interesting possibility. Incongruities between communication evaluations and source evaluations may be resolved by change in the evaluation of the communication, by change in the evaluation of the source, or both. Possibly when communications are not contradictory, changes in source evaluations are more likely to occur, and hence the displacement of communication ratings toward the source is less. However, when communications are contradictory to those on which previous source ratings are based, there is a greater tendency to maintain the original evaluation of the source and to displace communication ratings toward that evaluation. Congruity theory predicts that the relative magnitudes of changes in source and communication ratings can be estimated from their relative polarization (Osgood & Tannenbaum, 1955). However, the data could indicate that the relative changes in those ratings may also be affected by the degree to which characteristics of the source and characteristics of the communication are contradictory, independent of the extremity of the attitudes toward them.

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# EFFECTS OF ANTICIPATION OF FORTHCOMING INFORMATION ON PREDECISIONAL PROCESSES[1]

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The conflict-theory postulate that both bolstering activity and unbiased appraisal of alternatives may occur prior to the making of an overt decision was tested. The subjects had to choose between two unpleasant physiological stimulations. In one condition, subjects were told that prior to announcing their choice they would receive additional information about the side effects of the stimulations; in a second condition, subjects were told that there would be no additional information about the side effects. "Spreading" of the alternatives (bolstering) was found only in the no-information condition. The significant main effects for the information versus no-information variable support a conflict-theory assumption that under specifiable conditions spreading will occur before a decision is made. These findings are inconsistent with Festinger's (1964) assumption that spreading of alternatives will occur only after commitment to a decision has been made. A significant acceleration in heart rate during the period preceding the overt decision indicates that subjects found the decision stressful. The hypothesis that extraneous cognitive activity tends to increase the amount of bolstering was not supported.

A current issue in the psychology of personal decision making involves the extent to which people will be open-minded about alternative courses of action during the period preceding the making of a decision (Festinger, 1964; Janis & Mann, 1968; Jones & Gerard, 1967; Miller, 1968; Zajonc, 1968). Festinger and his co-workers (1964), in applying principles of dissonance theory to the study of decision processes, have opened up research on this problem. Their view is that prior to a decision people tend to be objective and unbiased in their appraisal of the pros and cons of each alternative; but after a decision is made there is a tendency to "spread" the alternatives by bolstering the chosen one or devaluing the other.

In an analysis of personal decision making from the standpoint of conflict theory (Janis & Mann, 1968), we have suggested that under certain conditions predecision bolstering occurs and may even facilitate arriving at a decision when the alternatives are almost equally attractive. We assume that before an overt committing decision is made there may be either bolstering activity or unbiased scanning of the alternatives, depending upon the presence or absence of certain incentive conditions. We expect that in the predecision phase a person will be motivated to remain objective and open-minded in considering the alternatives when he is alerted to the possibility of postdecisional conflict and regret should he fail to approach the decision carefully. On the other hand, the same person will tend to be subjectively wishful and biased in appraising the alternatives when he is concerned primarily with putting an end quickly to the current tension of reaching a decision.

Thus, we expect that undecided decision makers will sometimes attempt a resolution by distorting the attractiveness of the alternatives in order to avoid the unpleasant tension of predecisional conflict. One of the prime conditions under which we expect this outcome occurs when the supply of information about the alternatives appears to be exhausted, but there is still no clear-cut margin of preference on which to base a choice. Our hypothesis is

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that when the decision maker believes that he has obtained all the relevant information for making the decision that will be available to him and yet is not altogether confident about making a choice, he will tend to scan in a biased fashion the desirability of the more preferred alternative and will bolster it, so that it becomes increasingly more attractive than the less preferred alternative. This "spreading of the alternatives" is expected to occur predecisionally, before the person has committed himself, when he knows that he is still free to change his mind if he wishes. In contrast, when the individual is led to believe that there is the possibility of obtaining new relevant information right up to the time of stating his decision, he will anticipate the possibility of subsequent regret if he should prematurely bolster one alternative and therefore show little or no tendency to "spread the alternatives" before he is required to commit himself.

Another situation in which predecision bolstering may occur is when extraneous distractions interfere with the work of carefully thinking about choice alternatives. Because time and concentration are required for the objective appraisal of alternatives, such distractions may encourage a tendency to resort to superficial bolstering as a less effortful way of reducing predecision conflict. This hypothesis can be tested by occupying some subjects with irrelevant cognitive tasks during the predecision period, while allowing others to remain unoccupied.

In order to test these two hypotheses, we designed the following experiment.

## METHOD

### *Overview*

In a 2 × 2 factorial design, all subjects were required to decide which of two unpleasant physiological stimulations they would prefer to undergo. In order to test the effect of expecting additional information, subjects were led to believe that they would (or would not) receive information about the side effects prior to making their choice. In order to investigate the effect of extraneous cognitive activity on predecisional appraisal, one-half of the subjects in each of the two "information" conditions were kept mentally occupied, while the remainder were allowed to relax. The main dependent variable was the amount of change in appraisal of the alternatives during the predecision period.

### *Subjects*

Ninety female undergraduates from the University of Melbourne, Australia, were recruited as volunteers for an unspecified "psychological experiment" and were paid $1 each for their participation.

### *Procedure*

Subjects were ushered by a white-coated female experimenter into a "physiology" laboratory full of electronic equipment, tubes of chemicals, and a very conspicuous first-aid kit. They were told that the experiment had to do with the effect of physiological stimulations on the ability to carry out intellectual tasks and that they would undergo three different forms of physiological stimulation (movement, taste, and auditory stimulation) before being tested on simple arithmetic problems. Subjects were informed that each of the three stressors might possibly produce temporary side effects such as nausea, dizziness, headaches, and other unpleasant symptoms.[2] Next they were asked to rate the attractiveness of the three types of stimulation on a 15-point scale, for the purpose of helping the experimenter understand their performance of the intellectual tasks.

On completion of the ratings, electrodes from an electrocardiograph were attached to the subject's wrists, and the experimenter explained that heart rate would be measured to insure that she was completely relaxed before undergoing the physiological stimulations. Recordings of heart rate were taken at this point and at other points before, during, and after announcement of the decision.

### *Experimentally Manipulated Variables*

The procedure was varied to create two task settings. One-half of the subjects were kept preoccupied during the predecisional period with an irrelevant cognitive task (Concealed Figures Test). The other subjects were asked to try to relax during the waiting period. These variations were introduced to test whether extraneous cognitive activity interferes with predecisional appraisal of the alternatives. While the subjects "relaxed" or read the instructions of the Concealed Figures Test, the female experimenter left to check the movement apparatus which, she announced, had been giving trouble. The experimenter reappeared 2 minutes later, obviously agitated, and within earshot of the subjects telephoned a senior experimenter to report another breakdown in the movement apparatus. When the experimenter hung up, she told the subjects that movement stimulation had to be canceled and that there was now time for only one type of stimulation. They would be asked to choose between the two types for which the apparatus was still functioning, taste and auditory stimulation. After assuring them that there was no need to make up their minds for the present, the experimenter introduced the "information" manipulation. In one experimental condition, the subjects were led to expect additional information; the experimenter said that factual reports about the percentage of people suffering side effects from the different stimulations would be given to them later. In the no additional information condition, the subjects were told that, unfortunately, there was no information about the percentage of people suffering side effects.

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[2] At this stage two subjects refused to continue participation in the experiment. They were told about the purpose, paid by the experimenter, and then dismissed.

### *Revaluation of Alternatives*

Five minutes later, during the waiting period, the experimenter announced that it might be a good idea, because of the unexpected change in procedure, to obtain another rating of subjective feelings about the physiological tests. The subjects were told that they were not being asked to make their decision, and that the rerating was merely a precautionary repetition because of the unforeseen change in procedure. After this second predecisional rating, the additional information that had been promised to half the subjects was given. The subjects were told that from looking through the files it was now apparent that the same number of people had suffered side effects from the different stimulations. In order to hold constant the informational inputs in all conditions, exactly the same information was given to the subjects who had not been promised any additional information. The experimenter told these subjects that although she had thought no definite information was available on side effects a closer examination of the files made her realize that she could extract such information.

After receiving the information, each subject in each condition was asked to state her actual choice (of the taste or auditory stimulation) and this was recorded. Each subject was then asked to estimate her chances of suffering side effects from the chosen and unchosen alternatives and, as a final procedure, to rate how certain she felt about her choice. After this, the purpose of the experiment was revealed, and the experimenter asked a number of questions to determine whether there had been suspicions about any part of the experimental procedure.

## RESULTS

### *Predecision Bolstering*

Before making their decision, subjects rated the attractiveness of the alternatives twice: first, before knowing that a choice would be available, and second, after learning that there would be a choice and that information relevant to the alternatives would (or would not) be provided. In the present study, predecision bolstering is represented as a "spreading apart" in the value of alternatives, such that the to-be-chosen alternative increases in value and the not-to-be-chosen alternative decreases in value relative to each other. "Spreading" is computed by subtracting the difference between the alternatives on the first rating from the difference between the alternatives on the second rating, $(C_1 - N_1) - (C_2 - N_2)$, where $C_1$ and $C_2$ are the initial and final ratings of the to-be-chosen alternative, and $N_1$ and $N_2$ are the initial and final ratings of the not-to-be-chosen alternative.

### *Unstable Predecisional Ratings*

Of the 88 subjects, 25 showed a choice inversion; that is, they reversed the attractiveness of the alternatives from the first rating to the second rating, thus changing the rank order of the alternatives. This raises the question of the reliability of the ratings and the meaningfulness of the task. The experimenter's observational notes indicate, however, that the task was meaningful in that all subjects displayed considerable concern during the session and obvious signs of relief when dehoaxed. In some cases, reversal may have been due to the change in meaning of the alternatives or to the information about equal side effects, which might have made the initially preferred alternative appear to be less attractive. Several subjects said that they reversed because they had to attend a lecture immediately after the session and had begun to weigh the possibility of developing a headache from one of the stimulations. Table 1 compares the nonreversers and the reversers on initial ratings of the three physiological stimulations. Evaluations of the canceled movement stimulation were similar for both types of subject (7.70 versus 7.92), and therefore it is unlikely that nonavailability of this stimulation as a choice alternative differentially affected the reversers. Auditory stimu-

TABLE 1

Comparison between Reversers and Nonreversers on Initial Ratings of Alternatives

| Group | Stimulation | | |
|---|---|---|---|
| | Movement | Taste | Auditory |
| Nonreversers[a] | 7.70 | 5.44 | 5.68 |
| Reversers[b] | 7.92 | 5.08 | 4.48 |
| $t$ | .22 | .41 | 1.44 |

[a] $N = 63$.
[b] $N = 25$.

lation was rated lower initially by the reversers than by the nonreversers (4.48 versus 5.68), but the difference is not significant ($p > .10$). Initially, taste stimulation was rated slightly lower by the reversers (5.08 versus 5.44), but here too the difference is not significant ($p > .60$).

It is impossible to interpret the data on the bolstering effect for the reversers. (Which alternative should be considered as $C_1$ and which as $N_1$?) Nor can we analyze the "results" from these inconsistent subjects in a way that is comparable to dissonance experiments on decision making (see Brehm & Cohen, 1962; Gerard, 1967). Consequently, data from these 25 subjects were excluded from the main analysis. Approximately equal numbers of these excluded subjects came from each of the four experimental groups, specifically: additional information–preoccupied, $n = 7$; additional information–unoccupied, $n = 7$; no additional information–preoccupied, $n = 5$; no additional information–unoccupied, $n = 6$.

The subjects in each of the conditions gave essentially the same reasons for reversing their preferences. Hence, no obvious source of bias seems to have been introduced by eliminating the subjects who made unstable ratings. A subsidiary analysis which included the 25 reversers showed that inclusion of these unstable subjects added so much error variance that the main effects fell short of statistical significance, although the differences were in the same direction as those found in the data from the nonreversers.

### *Effects of Expecting More Information*

Table 2 reports the mean amount of predecision spreading for the two information conditions and the two preoccupation conditions, excluding those who showed unstable ratings. The first questions to be answered are whether the expectation of no more information leads to bolstering of the alternatives, and whether the expectation of more information inhibits bolstering. The bottom row in Table 2 shows that the mean spreading score was .81 in the no-information condition and .06 in the information-expected condition. An analysis of variance indicates that this main effect is significant at the .05 level ($F = 4.26$, $df = 1/59$). The magnitude of spreading in the no-information condition is significantly different from zero ($t = 2.84$, $p < .01$), whereas the amount of spreading in the information-expected condition is not significantly different from zero ($t = .31$, $p > .30$). Another complementary finding is that 38% of subjects "spread" the alternatives in the no-information condition, whereas only 10% did so in the information-expected condition. This 28% difference is statistically significant ($Z = 2.30$, $p = .01$).

TABLE 2

MEAN PREDECISION "SPREADING" OF ALTERNATIVES

| Preoccupation condition | No information expected | Information expected | $M$ |
|---|---|---|---|
| Unoccupied—free to think about decision | .47[a] | .20[a] | .33 |
| Preoccupied with extraneous cognitive task | 1.12[b] | −.06[c] | .55 |
| $M$ | .81 | .06 | |

[a] $N = 15$.
[b] $N = 17$.
[c] $N = 16$.

Table 3 shows that initially the ratings of the preferred and less preferred choice alternatives were very similar across the two information conditions. Accordingly, our pattern of experimental results is not attributable to differences between experimental conditions in the initial attractiveness of alternatives.

Although bolstering occurs in only a minority of the subjects, the results show that biasing tendencies do occur prior to the making of a decision. Moreover, the experimental re-

TABLE 3

INITIAL RATINGS AND CHANGES IN RATINGS OF ALTERNATIVES DURING THE PREDECISION PERIOD

| Condition | Alternative[c] | Rating 1 | Rating 2 | Difference | Predecision "spread" |
|---|---|---|---|---|---|
| No information expected[a] | TC | 6.09 | 6.78 | .69 | .81 |
| | NC | 5.00 | 4.88 | .12 | |
| Information expected[b] | TC | 6.26 | 6.13 | .13[d] | .06 |
| | NC | 4.90 | 4.71 | .19 | |

[a] $N = 32$.
[b] $N = 31$.
[c] TC = to be chosen, NC = not to be chosen.
[d] The difference score of .13 is in the wrong direction for "spreading."

sults provide support for the conflict-theory prediction that during the predecision phase the strength of the tendency toward bolstering will be relatively high or low, depending upon the presence or absence of situational conditions that affect the subjects' expectations of postdecisional regret. While the special conditions of the present experiment, including the elimination of those subjects who gave unstable ratings, limit the generalizability of these results, it is reasonable to conclude that biasing tendencies can be induced during the period preceding the overt decision.

### *Effects of Preoccupation Variable*

It was expected that because objective appraisal of alternatives requires time and concentration, interference with the work of carefully thinking about the alternatives would produce more bolstering. The analysis of variance results show, however, that the preoccupation variable did not produce a significant main effect ($F < 1$). The magnitude of spreading in the preoccupied condition (.55) is not significantly different from the amount in the unoccupied condition (.33). The interaction effect was also found to be nonsignificant ($F < 1$).

### *Physiological Reactions*

In order to assess emotional tension during the experiment, heart rate was measured at four time periods: (*a*) an 8-second interval following initial rating of the expected stimulations, (*b*) an 8-second interval (in the predecision period) 4 minutes after subjects had started the relaxation procedure or the Concealed Figures Test, (*c*) the 8-second interval coinciding with the decision, and (*d*) the 8-second interval which immediately followed debriefing. EKG records of three subjects were incomplete and had to be discarded.

Figure 1, which presents mean heart rate at each of the four periods, reveals a monotonic curve of increasing heart rate during the predecision period, reaching a peak during announcement of the decision. During the debriefing session there was a decrease in heart rate, which indicates subjects' relief at learning that they did not have to go through with the decision.

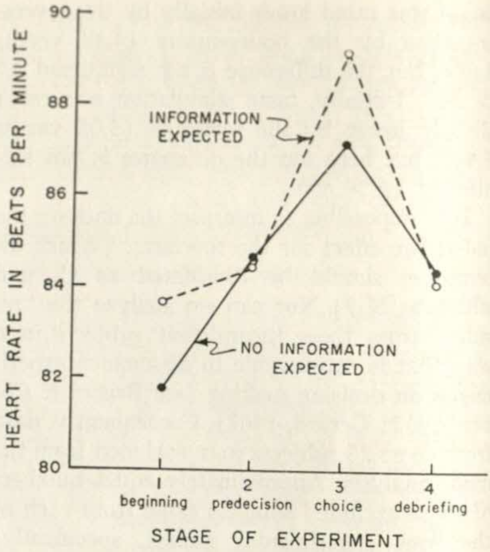

FIG. 1. Effects of information conditions on heart rate at four periods during the experiment.

An analysis of variance (Table 4) shows that neither of the two experimental variables had a significant effect on heart rate. But the results show a significant increase in heart rate from the period preceding the decision to the actual announcement of the choice ($F = 15.22$, $df = 1/56$, $p < .01$), which indicates that the demand to announce a decision acted as a stressor. These findings suggest that records of physiological arousal taken during the decision process may hold promise for the investigation of levels of stress and discomfort aroused by decisional conflicts (cf. Gerard, 1967).

TABLE 4

ANALYSIS OF VARIANCE FOR EFFECT OF PREOCCUPATION VARIABLE, INFORMATION CONDITION, AND STAGE OF DECISION ON HEART-RATE SCORE

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Between *S*s | 59 | | |
| Preoccupation variable (A) | 1 | 509.65 | 2.03 |
| Information condition (B) | 1 | 27.55 | <1 |
| A × B | 1 | 798.76 | 3.18 |
| *S*s within groups | 56 | 251.53 | |
| Within *S*s | 60 | | |
| Stage of decision (C) | 1 | 402.23 | 15.22* |
| A × C | 1 | 14.77 | <1 |
| B × C | 1 | 36.05 | 1.36 |
| A × B × C | 1 | 2.34 | <1 |
| C × *S*s within groups | 56 | 26.42 | |

* $p < .01$.

## DISCUSSION

In this study we have shown that at least under certain specifiable conditions bolstering tendencies do occur during the period preceding an overt decision. However, the question of predecision biasing is complicated by several closely related conceptual difficulties. It is often difficult to distinguish between the predecision and postdecision phase, mainly because many decisions are incremental in nature, the product of a series of small subdecisions. Further, there is the difficulty of investigating implicit decisions which the decision maker may arrive at long before he publicly announces his decision and which may be prematurely induced or distorted when an experimenter requires an overt expression of initial preference.

Since we had no control over implicit decisions made before the second rating, our study is only of marginal relevance to the question of whether biasing occurs before the making of an *implicit* decision. It could be argued, of course, that the crucial turning point may occur when the decision maker spontaneously arrives at an implicit decision, which could happen some time before the experimenter requires an overt expression of choice. According to this argument, the expectation of no more information induces the decision maker to regard his predecisional preferences as equivalent to a final decision, at which point he may begin bolstering the most preferred alternative (see Aronson, 1968; Festinger, 1964, p. 4; Weick, 1965). However, Festinger (1964, p. 42) has cited evidence from experiments by Jecker (1964) and others that does not appear to support this conception of the turning point. This evidence led him to conclude that bolstering or spreading of alternatives occurs only after a person is *committed* to a decision. Our findings pertain directly to this conclusion, since our subjects were unambiguously informed that they were not committing themselves at the time of the second (predecision) rating. Since the results do not support Festinger's conclusion that bolstering of the most attractive alternative occurs only after the person commits himself to the decision, they call into question the assumption that precommitment and postcommitment processes are dynamically different. The results of this experiment support the conflict-theory view that the amount of bolstering displayed by decision makers before making a decision depends upon the situational context, which partly determines the nature of the decisional conflict and the way it is likely to be resolved.

Elsewhere (Janis & Mann, 1968), we have shown how the evidence from the Jecker study and a study by Davidson and Kiesler (1964), which represents the main support for the position that predecision processes are objective, can be reconciled with the results of the present experiment. For example, in the Davidson and Kiesler experiment, in which no predecision bolstering was found, the subjects were told that it is impossible to reach any decent decision until they had read 10 items of information about the choice alternatives; but after receiving only 4 such items they were asked to make their decision. Hence the results can be interpreted as supporting the conflict-theory assumption that whenever more information is expected, decision makers will anticipate later conflict from a hasty decision based on incomplete information, and therefore will avoid bolstering the most attractive alternative. In Jecker's study, all the measurements were taken after the subjects had already made an overt choice between two desirable gifts. The so-called absence of predecisional spreading of alternatives was inferred from findings obtained under unusual postdecision circumstances that create considerable ambiguity; the subjects were told that they would receive both alternatives, so that their decision made no difference. It seems that Jecker's findings do not unambiguously support the conclusion that spreading apart of the alternatives cannot occur before a decision that commits the person to renounce one of the alternatives (see Janis & Mann, 1968, pp. 352–353).

Conflict theory assumes that in the period before the decision either unbiased appraisal of the alternatives or subjective bolstering may occur. When a person is trying to make up his mind, he may feel agitated partly because he anticipates that a faulty choice will trigger off even greater conflict in the future. If so, he is likely to be fairly objective and vigilant before making a decision. Under certain specified conditions, however, predecisional bolstering is expected to occur—as when the individual wants to put a quick end to his current conflict and has little concern about the possible negative consequences of not having thought through the decision objectively. Choice dilemmas sometimes arise—like the situation represented in the present experiment—in which all the information for making a careful decision seems to be at hand but there is no marked margin of preference for one alternative or another. Confronted with this dilemma, the conflicted individual may lapse into biased subjective thinking simply because a choice must be made and there is only limited information on which to base it.

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# AFFECTIVE REACTIONS TO EXPECTANCY DISCONFIRMATIONS UNDER PUBLIC AND PRIVATE CONDITIONS[1]

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The purpose of this study was to replicate and extend Carlsmith and Aronson's experiment of expectancy disconfirmations as a source of negative affect. The subjects were exposed to a procedure similar to Carlsmith and Aronson's under both public and private conditions. The results indicated that negative affect as a consequence of a strong expectancy disconfirmation is limited to public conditions. That is, for subjects in the public conditions only, the sweet solutions were rated less sweet and the bitter solutions more bitter on the disconfirmation relative to confirmation trials and incorrect trials. This was interpreted in terms of a social evaluation hypothesis although several modified dissonance approaches were not completely negated by the data.

Carlsmith and Aronson (1963) proposed that disconfirmation of any strong expectancy arouses dissonance. Assuming that dissonance is psychologically unpleasant, they predicted that events associated with strong expectancy disconfirmations would be judged less pleasant than the same events not associated with a strong expectancy. They tested this hypothesis by having subjects learn relationships between interpersonal cues and the presentation of bitter and sweet solutions. After stable expectancies had been established, the experimenter gave the cue for one solution and presented the subject with another solution. Comparing judgments regarding how pleasant or unpleasant the solutions were, the bitter solutions were rated as more bitter and sweet solutions less sweet on those trials when the subjects were incorrect with strong expectancies relative to when subjects were either correct or incorrect *without* strong expectancies. It was assumed that this degree of increased unpleasantness was a result of generalized negative affect initiated by dissonance arousal.

The purpose of the present study was to determine whether or not the above findings could be reasonably accounted for by some intervening process other than dissonance, as might arise from the social context of the experiment. Could the experimental manipulations have produced cognitions, for example, expectations of disapproval, that suggest alternative explanations for the obtained results? Let us examine the procedure closely.

To begin with, the subjects were given ego-involving instructions concerning the relationship between performance on the learning task and their personal sensitivity, perceptiveness, and insight. On each trial the subjects were presented with a cue and predicted the solution they believed associated with that cue. They were given the solution to taste, simultaneously told by the experimenter whether their prediction was "correct" or "incorrect," and then they rated the solution on its sweetness or bitterness. When the subject tasted the solution, therefore, he was aware of the fact that the experimenter knew how accurate he was. Carlsmith and Aronson noted the possibility that increased negative affect on "disconfirmation" trials after cue-taste expectancies had been built up might have been due, in part, to being incorrect. In view of the implications of being wrong, it is conceivable that a feeling of social rejection (rather than dissonance) caused increased negative affect. Nevertheless, Carlsmith and Aronson believed that the "incorrect"[2] trials could be used as controls since the subjects were wrong but were without a strong expectancy. Thus, greater negativity on the disconfirmation trials relative to the incorrect trials could only be accounted for by the presence of a firm expectancy.

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[1] The present study is based on a doctoral dissertation submitted to the University of Massachusetts. The author owes a special debt to David W. Lewit, chairman of the dissertation committee, for his invaluable suggestions, criticisms, and guidance. Appreciation is also extended to the other members of the committee: Robert Harrison, Bernard Berenson, and John O'Rourke. Thanks are also due to Walter Heimer and Helge Mansson for their comments on an earlier draft of this paper. Portions of this study were presented in summary form at the 1967 meetings of the American Psychological Association, Washington, D. C.

[2] "Incorrect" will be used to refer to wrong guesses before learning cue-taste associations, while "discon-

The existence of a stable expectancy may not have been the sole distinguishing factor, however, between the two events. On the disconfirmation trials the subjects had been right on at least two previous trials. No such criterion was employed on the incorrect trials. Where this varied expectancy strength for the subjects, it could have simultaneously altered the perceived expectancy strengths of the experimenter regarding their (the subjects') performance. Thus, subject expectancy strength may have been confounded with "perceived" experimenter expectancy strength. Being wrong on the disconfirmation trials may have been experienced as more unpleasant than being wrong on the incorrect trials, because the subjects perceived higher expectancy levels for the experimenter and hence believed the experimenter was more disappointed with their performance.

In the present experiment an experimental condition was employed wherein a subject's expectancies were *privately* disconfirmed. That is, some subjects were made to believe that only they were aware of their expectancies and only they were aware of subsequent confirmations and disconfirmations of these expectancies. Hence, subjective expectancies were confirmed and disconfirmed on a private level for some subjects and on a public level for others. On the basis of the author's interpretation of Carlsmith and Aronson's study, it was predicted that a disconfirmation of an expectancy would produce negative affect, but only when the subject believes the disconfirmation to be public. Specifically, the hypothesis was that subjects who experience expectancy disconfirmations under public conditions manifest increased negative affect, whereas subjects who experience expectancy disconfirmation under private conditions do not show any appreciable increase in negative affect.

## METHOD

### *Subjects and Design*

The subjects were 80 male undergraduates enrolled in general psychology at the University of Massachusetts. Each received $1 for his service as a subject. A 2 × 4 factorial design was used in which publicity and order of solution presentation were manipulated. One-half of the subjects were randomly assigned to the public condition and the other half, to the private condition. Within each of these groups there were four different orders of stimulus-solution trials. This order variable was included so that possible "sensory adaptation" effects could be observed and, if necessary, distinguished from the hypothesized psychological effects.

### *Apparatus*

The apparatus consisted of the following: (*a*) a two-switch stimulus box with a connecting four-colored-light stimulus panel, each toggle switch on the stimulus box being connected to a different three-light combination on the signal panel, and (*b*) a two-switch response box, with one switch marked "S" and the other "B," connected to a two-light response panel. Each toggle switch was wired to one of the two light signals on the panel.

The experimenter, who was separated from the subjects by a screen, had the stimulus box and response panel on the table before him. The subject had the signal panel and response box on a table before him. In addition, on a table in back of the experimenter and out of view of the subject, were 26 2-ounce paper cups. Each cup was filled to about one-half capacity with drinking water.

### *Taste Stimuli*

Two separate and distinctly different taste solutions were employed as the experimental stimuli. In line with Carlsmith and Aronson (1963), it was decided that solutions which elicited pleasant and unpleasant affective sensations would be proper for a systematic exploration of the stated hypotheses. A problem arose, however, in the application of the pleasant and unpleasant solutions employed by Carlsmith and Aronson. To begin with, they employed, as a sweet solution, 2 grams per liter of a saccharin solution. As a result, 10 of their 52 subjects were eliminated because they did not find the saccharin solution pleasant tasting. This same concentration was employed in a pilot investigation carried out by the present experimenter. In this study only two-thirds of the subjects reported the solution to be pleasant. In view of this, a solution other than saccharin was sought which would be pleasant tasting to as many persons as possible. Pfaffman (1958, 1960) reported that a 40% cane sugar concentration was judged a very pleasant-tasting solution by 100% of his subjects. Therefore, on the basis of the above evidence, it was decided that a 40% cane sugar solution would be employed as the pleasant-solution stimulus.

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firmation" will be used to refer to wrong predictions based upon learning.

A different problem arose when the selection of the unpleasant solution had to be made. Carlsmith and Aronson employed a quinine sulfate solution of .080 gram per liter as a bitter solution. Because the .080 gram per liter used by Carlsmth and Aronson was found to be overly unpleasant, a solution of .0325 gram per liter was decided upon.

### *Procedure*

Although the solution-rating instructions were the same as those employed by Carlsmith and Aronson, certain procedural adjustments were necessary so as to include a private condition. In general, those authors emphasized two "unrelated" experimental purposes—interpersonal sensitivity and psychophysical scale construction—whereas the present study was presented as one concerned with a phenomenon called "taste intelligence." Specifically, a private condition was employed wherein a subject could believe his expectancies and the accuracy of his predictions were unknown to others. This was accomplished by having the experimenter and subject communicate with each other via an impersonal signal system rather than with the aid of interpersonal cues and verbal responses.

When each subject entered the experimental room, he passed a table containing 26 small cups of water and was then seated at a table divided in half by a screen. He was told that the experimenter was studying something called "taste intelligence"—the ability of people to perform tasks that include learning to associate stimuli with various taste solutions. He was further informed that "taste intelligence" was positively related to superior ability and artistic judgment. He was then presented with the task instructions. Basically, the subjects were told that they would be presented with a light pattern to be followed by one of two solutions. They were to learn to associate a pattern with a solution. In addition, they were to taste and rate (on a magnitude estimation scale) each of the 26 different solutions (actually there were only two solutions—one sweet solution and one bitter solution).

Subjects in the public expectancy condition were informed that the experimenter would be fully aware of their performance on each trial. On the other hand, it was explained to the subjects in the private condition that the experimenter, because of the nature of the recording apparatus, would be unable to determine which or how many of their predictions were correct. He would know only at the end of the experiment how many "sweet" and "bitter" predictions had been made altogether. He would not get any information during the trials and would not be able to match predictions with outcomes.

Before the initial trial, each subject was asked to taste (and use as a comparison solution with a numerical value equal to 10) one sweet and one bitter solution. Each of the 26 trials that followed consisted of a cue, prediction, taste, rating, and, for those in the public condition, a statement by the experimenter as to whether their prediction was correct or incorrect. A 1-minute time interval was spaced between the presentation of the two standards and each trial thereafter.

The trials were categorized into "disconfirmation," "confirmation," "correct," and "incorrect" trials, on the basis of similar definitions employed by Carlsmith and Aronson. Any incorrect prediction preceded by two or more successive correct trials that occurred on Trial 9 or later was classified a "disconfirmation." Any correct prediction with the same prior specifications was classified a "confirmation."

Any correct prediction preceded by fewer than two successive correct trials or occurring before Trial 9 was termed a "correct" prediction. Similarly, any incorrect prediction preceded by fewer than two successive correct trials or occurring before Trial 9 was classified an "incorrect" prediction. All 80 subjects had at least one "confirmed," "disconfirmed," "correct," and "incorrect" prediction.

It should be noted that the subjects were made to believe that there were 13 distinctive sweet and 13 distinctive bitter solutions on the experimenter's table. While in reality the subjects tasted samples of only one sweet and one bitter solution, they believed that they had tasted a minimum of several of each kind of solution. Both the taste-estimation variability and the verbal responses of many subjects tended to confirm this.

### *Postexperimental Questionnaire*

Immediately after the twenty-sixth trial each subject was administered a questionnaire designed to check the assumptions and manipulations associated with the experiment. First, questions were asked concerning whether or not the sugar solutions were pleasant and the quinine solutions unpleasant. If subjects did not perceive the solutions to vary in pleasantness and unpleasantness, they were excluded from the analyses. Three subjects were excluded because they did not satisfy these criteria. In addition to a check on experimental assumptions, the questionnaire served as a check on the independent manipulation of publicity.

## RESULTS

### *Effectiveness of the Publicity Manipulation*

If the public-private manipulation was successful, subjects in the public condition should indicate that they perceived their performance to be potentially more public than subjects in the private condition. Ratings were made on a 6-point scale that covered the range from "everyone could know" to "nobody could know" about the subjects' behavior. An analysis by $t$ test confirms the effectiveness of the publicity manipulation. The mean rating of "potential publicity" was significantly greater ($t = 5.93$, $p < .001$) for subjects in the public condition ($\bar{X} = 2.42$).

TABLE 1

MEANS OF THE MEAN SWEET AND BITTER RATINGS FOR PUBLIC AND PRIVATE CONDITIONS

| Solution-trial category | Experimental condition | |
|---|---|---|
| | Public | Private |
| Sweet | | |
| Confirmation | 12.30[a] | 11.84 |
| Disconfirmation | 9.13 | 11.13 |
| Bitter | | |
| Confirmation | 17.45[b] | 16.00 |
| Disconfirmation | 19.63 | 12.89 |

[a] Higher score means more sweet.
[b] Higher score means more bitter.

TABLE 2

SUMMARY OF ANALYSIS OF VARIANCE FOR SWEET RATINGS

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Publicity (A) | 1 | .76 | 1.12 |
| Order (B) | 3 | .78 | 1.15 |
| A × B | 3 | 1.03 | 1.52 |
| *Ss*/AB | 72 | .68 | |
| Confirmation/Disconfirmation (C) | 1 | 3.35 | 11.55** |
| A × C | 1 | 1.49 | 5.13* |
| B × C | 3 | .31 | 1.06 |
| A × B × C | 3 | .11 | |
| C × *Ss*/AB | 72 | .29 | |

* $p < .05$.
** $p < .01$.

## *Major Findings*

One disconfirmation and confirmation rating score was obtained for each subject. These values, which were based on mean ratings for each category, were used as raw scores for all statistical comparisons.[3] The experiment, therefore, consisted of two between-subjects variables (publicity and order) and one repeated-measure variable (confirmation/disconfirmation). The use of confirmation-disconfirmation trial comparisons on the major analyses rather than incorrect-disconfirmation comparisons was based on the assumption that incorrect trials would occur much earlier in the sequence than disconfirmation trials, whereas confirmation trials would occur at about the same point as disconfirmation trials. With these analyses, therefore, confounding due to trial effects would be avoided.

The hypothesis concerning the relationship between publicity and affective responsiveness to disconfirmations was clearly supported. Increased negative affect was associated with public but not private expectancy disconfirmations.

Table 1 shows the means of the confirmation and disconfirmation sweet and bitter ratings for all cells in the design. Table 2 presents the analysis of variance for the mean confirmation and disconfirmation sweet ratings. The *F* ratio for the main effect of confirmation was significant. More important, however, was the significant interaction between the publicity and confirmation variables. Whereas the subjects whose predictions were private rated the sweet solutions about equal on confirmation and disconfirmation trials, subjects in the public conditions rated the sweet solutions as less sweet on the disconfirmation trials. A supplementary analysis of variance for simple effects showed that the public-sweet confirmation value ($\bar{X} = 12.30$) was significantly greater than the comparable disconfirmation value ($\bar{X} = 9.13$, $F = 15.00$, $p < .001$). No significant difference existed, however, between the two private-condition values for the sweet-solution ratings.

Table 3 presents the analysis of variance for the bitter ratings. The effect of the publicity variable was significant, with public ratings more bitter than private. The main effect for confirmation was not significant. The Publicity × Confirmation interaction was highly significant. The subjects' ratings in the public condition were higher on the disconfirmation trials ($\bar{X} = 19.63$) than on the

TABLE 3

SUMMARY OF ANALYSIS OF VARIANCE FOR BITTER RATINGS

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Publicity (A) | 1 | 10.00 | 5.81* |
| Order (B) | 3 | 1.69 | |
| A × B | 3 | .74 | |
| *Ss*/AB | 72 | 1.72 | |
| Confirmation/Disconfirmation (C) | 1 | .20 | |
| A × C | 1 | 4.13 | 14.75** |
| B × C | 3 | .20 | |
| A × B × C | 3 | .39 | 1.39 |
| C × *Ss*/AB | 72 | .28 | |

* $p < .05$.
** $p < .01$.

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[3] A square-root transformation was applied to the data in order to make the within-cell distributions approximately normal and homogeneous in variance.

confirmation trials ($\bar{X} = 17.45$, $F = 4.59$, $p < .05$), whereas the ratings for subjects in the private condition were lower on the disconfirmation trials ($\bar{X} = 12.89$) than on the confirmation trials ($\bar{X} = 16.00$, $F = 11.44$, $p < .005$).

It should be noted that, unlike Carlsmith and Aronson's study, the above-mentioned analyses are based on comparisons between disconfirmation and confirmation trial judgments. As it turned out, contrary to expectations, the disconfirmation trials occurred significantly earlier ($\bar{X} = 16.9$) than the confirmation trials ($\bar{X} = 18.2$, $A = .064$, $p < .001$).[4] Although this is a reliable difference, it seems unlikely that a difference of about 1 trial over a total of 26 trials would cause any meaningful changes in negative affect. If anything, one would expect increased negativity during later trials due to such factors as fatigue and boredom.

Carlsmith and Aronson used as their "crucial" comparisons the difference between being incorrect with an expectancy (disconfirmation) and being incorrect without an expectancy (incorrect). In view of the above-mentioned trial effect, and in order to examine as closely as possible the relationship between their data and the results of the present study, analyses were carried out with similar incorrect[5] and disconfirmation ratings in the public and private conditions. Ratings for sweet solutions in the public condition were significantly greater on the incorrect trials ($\bar{X} = 11.5$) than on the disconfirmation trials ($\bar{X} = 8.6$, $A = .136$, $p < .01$), whereas the private-condition comparison yielded no difference between incorrect ($\bar{X} = 10.2$) and disconfirmation ($\bar{X} = 11.1$) trial ratings. Similarly, public-condition comparisons for bitter ratings revealed significantly lower incorrect trial ratings ($\bar{X} = 15.6$) than disconfirmation ratings ($\bar{X} = 18.6$, $A = .168$, $p < .01$). On the other hand, the same private-condition comparisons between incorrect ($\bar{X} = 14.1$) and disconfirmation ($\bar{X} = 12.7$) trials showed no significant difference existed between the two types of trials. These analyses, more in line with those carried out by Carlsmith and Aronson, give further support to the hypothesis that increased negative affect is associated with expectancy disconfirmation under public conditions only.

Finally, findings bearing on the influence of "sensory adaptation" on the subjective ratings were examined. No significant main effects or interaction effects involving the order variable were found. Although some adaptation may have actually occurred, the order in which the solutions were presented to the subjects played, at most, a minimal role in determining solution estimations on the confirmation and disconfirmation trials.

## DISCUSSION

The overall results of this experiment provide strong support for the hypothesis that disconfirmation of expectancies leads to negative affect only when the disconfirmations are subject to public knowledge. In view of these findings, the assumptions of Aronson, as found in Brehm and Cohen (1962), and of Carlsmith and Aronson (1963) are clearly to be qualified. They concluded, it will be recalled, that "if a person expects a particular event (X) and instead a different event (Y) occurs, he will experience dissonance [Carlsmith & Aronson, 1963, p. 153]." Dissonance, therefore, was seen as a direct manifestation of the disconfirmation of any strong expectancy. Furthermore, this cognitive dissonance was measured by the amount of negative affect (unpleasantness) that generalized to environmental stimuli, for example, sweet and bitter solutions. On the basis of the results found in the present study, there is strong evidence in favor of limiting the generality of the expectancy hypothesis. That is, under conditions where both expectancies and outcomes are public, if an individual expects a particular event, X, and a contrary event, Y, occurs, he will perceive Y to be less pleasant than if either X had actually occurred or if Y occurred without any associated expectancy.

These results—that taste judgments are altered by disconfirmation only in the public condition—imply either (*a*) that dissonance occurs only in that condition or (*b*) that some other mechanism associated with publicity accounts for the judgmental alteration.

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[4] Sandler's $A$ is statistically equivalent to a correlated $t$ test (Runyon & Haber, 1967).

[5] Incorrect trials occurring on Trial 9 or later were used in these analyses. This was done to minimize the influence of trial effects. Since not all subjects manifested sweet and/or bitter incorrect trials after Trial 9, the following $N$s were used: sweet public, $N = 19$; sweet private, $N = 17$; bitter public, $N = 25$; bitter private, $N = 16$.

### *Dissonance Interpretation*

Dissonance may explain the data if it is assumed that performance under private conditions is not subjectively important. According to Festinger (1957), the amount of dissonance aroused is a function of the importance of the associated cognitive elements. Removal of the social evaluation element may have, at the same time, significantly reduced the individual's perception of the importance of his own performance. This assumption regarding the role of importance of cognitive elements in dissonance arousal has experimental support (Deutsch, Krauss, & Rosenau, 1962; Zimbardo, 1960). Furthermore, Cohen, Brehm, and Latané (1959) replicated a gambling experiment by Festinger (1957, pp. 126–131) and introduced publicity by making the subjects believe their winnings would be made public. The authors assumed that publicity regarding one's performance served to enhance the perceived importance of the performance. Finally, in a more directly relevant study, Sampson and Sibley (1965) concluded that, for bitter taste judgments at least, ego involvement is necessary for disconfirmation to produce negative affect.

To relate the results to the "importance" assumption of dissonance theory, all subjects were asked to rate, on a 5-point scale, how important they felt their performance on the task was. A $t$ test applied to these ratings revealed that subjects in the public condition attributed greater importance to their performance than those in the private condition ($t = 2.34$, $p < .05$).

Pearson correlations were also computed between subjects' importance ratings and adjusted disconfirmation ($D$) scores. These values were obtained by subtracting each subject's ratings on "incorrect" trials from his ratings on the "disconfirmation" trials. They were arranged in such a manner that a positive $D$ value indicated greater negative affect for the disconfirmation trials. Correlation coefficients for the private and public conditions and sweet- and bitter-solution trials were obtained. All four were positive in direction with the sweet trial correlations reaching statistical significance (private $r = .67$, $p < .01$; public $r = .75$, $p < .001$). Neither of the bitter trial correlations was statistically significant (public $r = .06$, *ns*; private $r = .24$, *ns*).

On the surface, these relationships based on "importance" data support a dissonance interpretation of the data. They are, however, ad hoc ratings, and thus may not reflect the subject's perceived importance of the task while he was tasting and rating the solutions.

Another possible dissonance interpretation is that subjects in the private condition perceived the experiment as trivial. Since the accuracy of their judgments was not available to the experimenter, they may have concluded that he could not really learn anything about taste intelligence from their performance. Thus, no dissonance existed for these subjects because, for them, the experimental surrounding lacked any degree of significance.

Finally, a problem characteristic of many "dissonance" experiments is particularly salient in the present study. Dissonance, being a hypothetical construct, is generally measured indirectly through dissonance reduction. Investigators, on the basis of such criteria as interest, relevance, and feasibility, choose to measure one or perhaps even several "dissonance-reducing" responses. In the present study, increased negative affect was used as the measure of dissonance arousal. This does not mean, however, that all subjects who experienced dissonance manifested greater negativity. In fact, subjects in the private and public conditions may have experienced the same general level of dissonance arousal only to express such arousal in two different ways. Those in the public condition may have expressed dissonance by rating solutions more negatively, whereas subjects in the private condition, knowing that the experimenter could not measure their taste intelligence, may have devaluated the importance of the experiment. Hence, as is the case in many dissonance studies, behavioral variations may, in reality, merely reflect the measurement of a selective response as well as differences in response tendencies facilitated, in part, by experimental manipulations.

### *Negative Social Evaluation*

One obvious difference between conditions is that subjects in the public condition were aware of the fact that the experimenter knew if their predictions regarding the solution were correct or incorrect. Hence, in the public condition only, the subjects were publicly incorrect on the disconfirmation trials. It is known that being incorrect in public is an obvious source of displeasure for most people in today's society. Furthermore, for college students who are sensitive to the evaluations of others regarding their performance this displeasure can be magnified when any direct or indirect negative evaluation of their performance arises. Therefore, the increase in negative affect from confirmation and incorrect to disconfirmation trials that occurred in the public condition only may have been due to the subjects' feelings of displeasure arising from an implicit negative evaluation of their performance by the experimenter. The fact that these differences occurred only in the public condition is consistent with the interpretation, presented earlier, that the experimenter's perceived expectancy strength has a significant impact on an individual's observation and evaluation of his own performance.

In conclusion, it seems clear that a negative-evaluation interpretation is more parsimonious since one need only assume that subjects are conditioned concerning failure by an authoritative evaluator. Thus it becomes unnecessary to postulate an aversive drive such as dissonance in order to account for negative affect being associated with a public event.

There are a number of possible approaches that could be taken so as to determine the appropriateness of these two interpretations. The relevance of cognitive importance could be investigated by the direct experimental manipulation of importance via a means other than publicity (e.g., specifically calling attention to how important the experiment is to psychologists). In this way the relationship between importance and affective responsiveness to expectancy disconfirmations could be more directly determined.

The social censure hypothesis could be studied by exploring the nature of the social agent's—in this case, the experimenter's—relationship with the subjects. The work by Orne (1962) and others has already pointed out the degree to which subjects comply with the perceived wishes of experimenters. In addition, Rosenberg (1965) has argued that subjects are highly motivated to be evaluated positively by experimenters. If, within the present context, social evaluation does affect their behavior, then public knowledge of their performance by "nonsignificant others" should produce little or no negative affect following disconfirmations.

Finally, experiments in the direction of more socially meaningful behaviors are needed in the future. Those already conducted and relevant to the present discussion, such as a recent one by Aronson and Linder (1965), illustrate what may be presently concluded—individuals respond most positively to reactions by others which raise their self-esteem or maintain it at an already high level. In other words, confirmation or disconfirmation of expectancies regarding social behavior apparently takes a back seat to the raising or lowering of self-esteem.

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# MAINTENANCE OF SELF-ATTRIBUTED AND DRUG-ATTRIBUTED BEHAVIOR CHANGE[1]

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It is proposed that behavior changes which are believed to be brought about by oneself will be maintained to a greater degree than behavior changes which are believed to be due to external forces or agents. Within the framework of psychoactive drug therapy, a change in overt behavior which is attributed to one's own efforts should be more persistent than a change in overt behavior which is attributed to a drug. An experimental analogue of drug therapy is reported in which subjects (*a*) underwent a pain threshold and shock tolerance test, (*b*) ingested a drug (really a placebo), and (*c*) repeated the test with the shock intensities surreptitiously halved. All subjects thus believed that a drug had changed their threshold performance. Half of the subjects were then told that they had actually received a placebo, whereas the other half continued to believe that they had received a true drug. It was found that subjects who attributed their behavior change to themselves (i.e., who believed they had ingested a placebo) subsequently perceived the shocks as less painful and tolerated significantly more than subjects who attributed their behavior change to the drug.

This research is concerned with a person's attributions about the causes of a change in his behavior. That is, given that a person is behaving in a particular manner, will the cognitive and behavioral consequences be different if the cause of it is attributed to himself than if it is attributed to an external force or agent? It will be hypothesized that behavior change which is attributed to oneself will persist or be maintained to a greater degree than behavior change which is attributed to an external agent such as a drug.

Although this research is restricted to overt behavior, much of it was stimulated by considerations of covert visceral behavior. If we define behavior as a perceptible physiological reaction, there is good evidence that different attributions about the source of this "internal behavior" will have different effects. Schachter and his associates (Nisbett & Schachter, 1966; Schachter & Singer, 1962) have clearly shown that if this internal behavior is attributed to oneself, the consequences will be different than if the behavior is attributed to an external agent such as a drug. When it is attributed to oneself, the person has some information which can lead to inferences about himself or about stimuli in the external environment. Thus, if *I* have reacted in this anger-inducing situation, then I must be angry. If *I* have reacted in this euphoric situation, then I must be happy. If *I* have reacted to these electric shocks, then they must be painful. The situation is quite different, however, if the physiological reactions or internal behaviors are attributed to an external agent. The person is less able to make inferences about the external situation or about himself if his internal behavior is drug-induced. Thus, subjects do not get angry or happy (Schachter & Singer, 1962) and do not consider shocks painful (Nisbett & Schachter, 1966) if they believe that their internal behavior is drug induced. In effect, a self-attributed visceral reaction makes us stop, look, and listen, and allows us to make certain inferences about ourselves and the stimulus situation. A drug-attributed visceral reaction provides us with little or no information about ourselves or the world.

These speculations about attribution are relevant to a psychiatric problem which is particularly serious but which unfortunately is far too often ignored. That is, how can psychiatric patients be weaned off tranquilizers? The remarkable effects that the introduction of tranquilizers has had upon the treatment of psychiatric patients is well known. It has been estimated, for example, that the patient population of mental hospitals in New York State, which for some time had been increasing by 2,000 or more every year, dropped by 500 through increased discharges during the first year in which chlorpromazine and reserpine were in general use (White, 1964). Within the hospitals still further consequences could be observed (for example, 75% fewer patients being placed on restraint or in seclusion).

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Without denying the importance of these drugs, a curious problem has arisen in that it has become increasingly evident that drugs often cannot be withdrawn without causing patients to relapse (Kamano, 1966). Although drugs produce significant and beneficial behavioral changes, these changes seem to be dependent upon the continued use of the drug. When the drug is terminated, the person's behavior deteriorates. Psychiatrists have not adequately responded to this problem. The general practice is apparently to maintain patients on drugs indefinitely. Apart from the possibly adverse physiological effects of long-term drug usage, the problem is of interest for theoretical reasons. It seems to indicate that little if any relearning occurs on the part of people who take these drugs. That is, if a patient is chronically anxious or frightened by a number of things in his environment, he can be calmed down, perhaps indefinitely, by tranquilizers, but the situations that set him off originally will do so again once the drug is withdrawn. The patient has not learned that the situations are harmless.

A similar problem is evident in the animal literature. A number of studies, in particular those of Neal Miller and his associates (Miller, 1966), have found that drugs will reduce fear as evidenced by the reinstatement of a bar-pressing response which was previously associated with electric shock, but that once the drug is withdrawn, the animals stop the bar pressing. The fear response is evidently suppressed temporarily by the drug, but no relearning is apparent since it reappears once the drug is withdrawn. The animals evidently have not learned that bar pressing is no longer associated with shock. Although the animal literature is by no means consistent (e.g., Miller, Murphy, & Mirsky, 1957; Nelson, 1967), it can be summarized by saying that the transfer of a drug-produced change in behavior to the nondrug state is often difficult to find.

Granted that the problem exists and that it warrants further attention, how can we get a drug-induced change in behavior to persist into the nondrug state? Considering the experiments dealing with the labeling of visceral behavior, the answer may be quite simple. What would happen if we made someone attribute his changed behavior to himself rather than to the drug? Imagine a patient whose persistent fears have diminished due to a tranquilizer that he has been taking for some months. After this period let us convince the patient that we have substituted an inert placebo for the real drug throughout the past month. What should he conclude from this? We believe that the patient would now have to accept the responsibility for his changed behavior and because of this might make three inferences: (*a*) "The world can't be that frightening after all" (he will reevaluate the stimulus situation); (*b*) "I have succeeded and am competent" (he will feel happy and proud of himself); (*c*) "I will subsequently be able to behave differently when in stressful situations" (his expectations and aspirations for improved behavior will be higher). These kinds of inferences would seem to facilitate the maintenance of the behavior change once we have actually terminated the drug. Such subjects should maintain their improved behavior to a greater degree than those who have a drug-attribution for their behavior change.

These attribution-notions lead to the following hypothesis: Subjects who are disabused of the cognition that a drug has produced certain significant changes in their behavior will be more likely to maintain these behavior changes than subjects who continue to believe that their changed behavior was due to a drug. A direct way of testing this hypothesis would be to use drugs, but for several reasons we chose not to: they require extensive pretesting; they are relatively unpredictable; and the supervision of a physician would be necessary. Additional difficulties are posed by the necessity to establish a dosage which would be strong enough to change behavior yet weak enough to convince a subject that he had "actually" received a placebo. While all these problems do not appear insurmountable, our primary interest in attribution per se rather than in drugs led to an analogical test of the hypothesis. The experiment was therefore conducted by (*a*) obtaining a premeasure of pain and tolerance thresholds using fingertip-administered electric shocks; (*b*) administering a placebo capsule which was presumably a drug; (*c*) surreptitiously lowering the overall intensities of the shocks during a second threshold procedure so that all subjects believed that they could take more electric shocks before pain was experienced and before they desired to stop than they did during the first threshold procedure; (*d*) telling half the subjects that they had received a placebo; and (*e*) assessing their reactions during a third threshold procedure during which the subjects received no pill. It was predicted that those subjects who were led to attribute their behavior during Threshold II to themselves (because they had received a placebo) would subsequently raise their thresholds more than subjects who attributed their behavior during Threshold II to a drug.

## STUDY I

### *Method*

*Subjects.* Subjects were male undergraduates enrolled in introductory psychology courses. As part of the course requirements, they were offered participation in two separate ½-hour experiments to be run back-to-back. The first was described as an experiment in skin sensitivity and the second as an experiment in intersensory stimulation. Subjects had no expectations about the use of electric shock or drugs.

*Apparatus.* The shock generator delivered 60 cycle shocks, 500 milliseconds in duration. The shock intensities for the first and third threshold tests began with 0 microamperes and increased by 200 microamperes at each step. The intensities for the second threshold test began with 0 microamperes and increased by 100 microamperes at each step. The electrodes were Grass sliver-plated cup-type, $\frac{5}{16}$ inch in diameter, taped with electrode paste to the first and third fingertips of the subject's nondominant hand.

*Procedure.* Structured to the subject as two independent experiments, the procedure actually entailed three shock-threshold tests, the first two administered by one experimenter and the third by another. The procedures were identical for all subjects except where noted below. The design is summarized in Table 1.

After the usual introductions, the experimenter explained that he was studying the possible effects of a vitamin compound, "parataxin," upon skin sensitivity. In order to test this, he would determine, by means of a specially constructed shock apparatus, when the subject first perceived a shock, first felt that it was "painful or unpleasant," and finally perceived it as "so painful or unpleasant that you cannot tolerate any more." A parataxin capsule would then be administered and a second threshold taken a few minutes after ingestion of the "fast-acting drug." The subject was assured that both the pill and the shocks were quite harmless. At this point each subject was asked to sign a "Subject Consent Form," which indicated that a full explanation of the experiment would be given later and that the subject could withdraw at any time. No subject withdrew from the experiment.

After attaching the electrodes the experimenter gave the subject a score sheet on which to check the shocks as they were administered; it was intended thereby to make the manipulated changes at Threshold II all the more salient. One hundred shock intensities were listed, and the subject was instructed to circle those corresponding to the three threshold points described above. The first threshold test was then administered. Following this test, the subject was given the parataxin capsule and left alone "while it takes effect." After 2 minutes the experimenter returned and repeated the threshold test, except for a surreptitious manipulation of the actual shock intensities designed to make the subject believe that the capsule had changed his threshold

TABLE 1

SUMMARY OF EXPERIMENTAL DESIGN

| Group | Procedure | | | |
|---|---|---|---|---|
| Placebo | Threshold I (Experimenter I) | Threshold II (Experimenter I) with pill | Disabuse: "It was a placebo" | Threshold III (Experimenter II) |
| Drug | same | same | No disabuse: "Now the drug has worn off" | same |

performance. The shock intensities were halved such that, for example, when the subject received Shock Number 5 on this second threshold test, he was actually receiving half of what he had received on Threshold I for "Shock Number 5."

Up to this point the experimenter was "blind" as to experimental condition. He now flipped a card from a hidden pile of group-assignment cards, telling him whether the subject was to be in the "placebo" or "drug" group. At this point instructions varied as follows:

> *Placebo:* Would you be very surprised if I told you that you didn't get a real drug? We really just gave you a placebo pill; that is, the capsule just contained inactive sugar. We had to do this to control for what is called the placebo effect. When you do drug research, sometimes people are affected just because of the attention that you have given them, and regardless of what you have given them. To control for this effect we have divided our subjects into two groups; one group gets the real drug and another group gets the fake pill. To determine whether the drug has had an effect, we have to compare drug people with placebo people. Do you understand?

The subject's questions as to why he had taken more shock were deflected by statements like, "Well, that could happen, I guess." The subject was then sworn to secrecy and thanked for his participation.

> *Drug:* Let me check your eyes to see to what extent your pupils can dilate. This is a measure of the amount of effect the drug has had on you. I am just going to shine a small light in your eye, and there is nothing to be afraid of. Just hold your head steady, please. Your pupils are still not constricting completely. The parataxin is beginning to wear off though, and you should be back to normal in another minute or so.

After a second pupillary check, the experimenter said that the parataxin had, in fact, worn off. The subject was then thanked for his participation.

*Threshold III.* The second experimenter entered shortly after the first had left, but before he could launch into the cover story for "his experiment" the first experimenter returned and asked the subject to fill out a questionnaire which "the Department requires in experiments which use drugs." The second experimenter suggested that the subject do this during a natural pause in the experiment. (This ploy resulted in subject's construing the questionnaire as part of the "first experiment" without, however, his actually seeing it until after Threshold III.) The second experimenter then proceeded to explain that he was studying the effects of intersensory stimulation upon pain sensitivity. He was running his study in tandem with the first experiment simply because of the availability of the apparatus and for convenience of scheduling. For purposes of experimental design, the subject would have to undergo another threshold test "which you probably are already familiar with from the first experiment." After this initial threshold test, the subject would be given another, during which auditory stimulation would be interspersed. The same threshold test as in Threshold I was then administered. Naturally, this experimenter was "blind" as to the subject's condition and performance. Following this, the subject completed the questionnaire. Prior to debriefing, the last nine subjects (five placebo and four drug) were asked for a rating of a sample shock. The shock administered was that which the subject had indicated as "painful or unpleasant" on Threshold I, this bit of information having been surreptitiously left behind by the first experimenter. Several further questions were asked of all subjects before complete debriefing, namely (to placebo subjects): "I see that you took more shocks on Mr. ________'s second threshold test. Since the capsule was actually a placebo, how do you account for this?" To all subjects: "Did you see my experiment related in any way to Mr. ________'s?" "How many shocks did you expect to take on the threshold test I just gave you?" All subjects were then thoroughly debriefed.

### *Results*

*Success of the manipulations.* The questionnaire which the subjects had filled out "for the Department" provided supporting data that the desired effects had been achieved by the various manipulations. All subjects in the placebo group did, in fact, regard the pill they had received as a placebo and, except for two subjects, the drug subjects believed that their capsule was a true drug and that it had produced a change in skin sensitivity. The data of two drug subjects, who suspected that they had gotten a placebo and that the shock intensities had been halved, were excluded. None of the subjects considered the "two experiments" as related.

*Threshold performance.* It should be noted first that all subjects took more shocks on Threshold II than on Threshold I before announcing pain and tolerance. There were also no differences between placebo and drug groups when Threshold II was compared to Threshold I ($t$'s $< 1$). Thus, the two groups of subjects behaved similarly through the second threshold test, as one would expect from the random assignment of subjects to the groups.

The data of primary interest are the differences between Threshold III and Threshold I for those points at which subjects reported pain and inability to continue. As can be seen in Table 2, subjects who were disabused of the cognition that they had received a drug (placebo) took an average of 1.77 more shocks at the third threshold test before reporting pain, while subjects who

TABLE 2

SHOCKS TAKEN BEFORE ANNOUNCING PAIN AND TOLERANCE, STUDY I

| Condition | $N$ | Pain | | | | | Tolerance | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | No. shocks | | | Difference | % improvement | No. shocks | | | Difference | % improvement |
| | | I | II | III | III–I* | III–I/II–I*** | I | II | III | III–I*** | III–I/II–I** |
| Placebo | 13 | 6.69 | 10.31 | 8.46 | 1.77 | 75% | 13.08 | 24.08 | 14.23 | 1.15 | 10% |
| Drug | 11 | 7.82 | 13.00 | 7.82 | 0 | −4% | 11.73 | 19.91 | 11.00 | −.73 | −26% |

* $p = .12$, two-tailed Placebo versus Drug.
** $p = .06$, two-tailed Placebo versus Drug.
*** $p < .05$, two-tailed Placebo versus Drug.

entered the third test still believing that the drug had effected the changes during Threshold II did not, on the average, improve at Threshold III as compared to Threshold I. The difference between the improvement of placebo and drug subjects, however, is marginally significant. More striking are the changes for tolerance. Here placebo subjects endured 1.15 more shocks, while drug subjects took −.73, this difference being statistically reliable.

Perhaps a better way of looking at the data is to consider the improvement from Threshold I to III as a percentage of the improvement from I to II. Analyzing the data in this manner allows us to evaluate the percentage of "drug-improved" behavior which is subsequently maintained. Such an analysis also has the virtue of correcting for different degrees of behavior change from Threshold I to II. It can be seen in Table 2 that placebo subjects maintained a greater percentage of their "drug-improved" behavior than did drug subjects. The principal hypothesis was therefore confirmed: disabusing subjects of the cognition that a drug had effected improvement in their shock-taking behavior leads to greater generalization of this "drug effect" to the "undrugged" state.

Some additional findings are also of interest. It will be recalled that following Threshold III the subject was asked how many shocks he had thought he would take on that threshold. Of the 13 placebo subjects 10 (77%) indicated that they had expected to take more shock on the third threshold than on the first, while of the 11 drug subjects none expressed the expectation of taking more. This difference is highly significant ($p < .001$), suggesting that being disabused of a drug-attribution cognition increases subjects' expectations about the number of shocks they will be able to take on a subsequent test.

It will also be recalled that nine subjects were asked, following Threshold III, to rate a single shock on a scale ranging from 0 (not at all painful or unpleasant) to 100 (extremely painful or unpleasant); the shock given each subject was that rated as painful on Threshold I. Analysis of these data reveals that placebo subjects considered this shock to be somewhat less painful than did drug subjects ($t = 2.15$, $p < .10$, two-tailed). All subjects also rated the perceived pain of the last shock that they had taken on the final threshold. In spite of the fact that the placebo subjects had taken stronger shocks, they rated their final shocks as being no stronger than did the drug subjects. These two sets of ratings suggest that the placebo subjects reevaluated the painfulness of the shocks.

## STUDY II

### *Method*

A replication was conducted with an apparatus which permitted the delivery of shocks in smaller incremental steps. Thresholds I and III now consisted of a series of shocks increasing by 100-microampere steps while Threshold II increased in 50-microampere steps. It was thought that by using smaller increments the dependent measure would more readily reflect the subjects' ability to withstand shock. The electrodes and their placement were the same as in Study I. An added feature was a remote control unit that enabled the experimenter to deliver the shocks at some distance from the subjects, as well as to reduce by one-half the intensities of all shocks for Threshold II. As in the first study, all shocks were 500 milliseconds in duration.

This replication was identical to Study I except for a procedural change designed to improve the deception. The subject was told that *he* would have to set the shock intensities himself under the ruse that the experimenter had to monitor various physiological changes at a console at the other end of the room. It was felt that the deception would thereby be even more compelling since the subjects would now be less likely to think that the experimenter was reducing the intensity of the shocks. Dummy electrodes were taped to the forearm of the subject's nonpreferred hand for this purpose, and the shock box was positioned so that the subject could read and manipulate the decade switches. The instructions were to increase each successive shock by 100 microamperes. In all other respects the procedure followed by the first experimenter was the same as that of the first experiment. With the exception that the subject was setting the shock intensities, the second experimenter also followed the same procedure as in the first experiment.

### *Results*

As in Study I, the manipulations were successful. Placebo subjects regarded their pill as a placebo while drug subjects believed it was a drug and attributed their improvement at Threshold II to it. Furthermore, all subjects noted significant decreases in their pain sensitivity at Threshold II and all believed that the two experiments were unrelated. The data of one drug subject were excluded, however, because he thought that the drug was still active during the third threshold test.

*Threshold performance.* As in Study I, the first and second thresholds of the two groups did not differ. The placebo and drug groups did differ, however, in their change-scores between Threshold III and Threshold I for reports of pain and of tolerance. It can be seen in Table 3 that once again there is marginal significance for pain, the placebo subjects showing a mean increase of 4.54 shocks as compared to the increase of 1.10 shocks of the drug group. If we combine the probability levels for Studies I and II (cf. Stauffer et al., 1949), the marginal differences for pain are significant at the .03 level. Considering the point at which subjects stopped taking shock, placebo subjects also raised their tolerances significantly more than drug subjects. Analyzing the improvement on Threshold III as a function of the improvement on Threshold II leads to similar data: placebo subjects maintained a greater percentage of their "drug-improved" behavior than did drug subjects.[2]

As in Study I, subjects were asked how many shocks they had expected to take on Threshold III. Of the 11 placebo subjects 9 (82%) expected to take more shock than they had on Threshold I, while of the 10 drug subjects none had this expectation. This difference is highly significant ($p < .001$), replicating the corresponding finding from Study I.

Subjects were also asked to rate a single shock on a scale from 0 to 100, the shock given being that which had been rated as painful by each respective subject on Threshold I. Whereas marginal significance had been found in Study I (placebo subjects tending to rate this as less painful than drug subjects), the difference in this replication was not significant. This finding is also supported by the ratings of the last shock of the third threshold. Placebo subjects rated this shock as significantly *more* painful than did drug

TABLE 3

SHOCKS TAKEN BEFORE ANNOUNCING PAIN AND TOLERANCE, STUDY II

| Condition | $N$ | Pain | | | | | Tolerance | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | No. shocks | | | Difference | % improvement | No. shocks | | | Difference | % improvement |
| | | I | II | III | III–I* | III–I/II–I* | I | II | III | III–I** | III-I/II–I** |
| Placebo | 11 | 10.40 | 26.40 | 14.98 | 4.54 | 37% | 21.27 | 48.00 | 25.27 | 4.00 | 15% |
| Drug | 10 | 10.20 | 24.90 | 11.30 | 1.10 | 13% | 21.60 | 43.60 | 20.80 | −.80 | 4% |

* $p = .12$, Placebo versus Drug.
** $p < .01$, Placebo versus Drug.

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[2] Moreover, we find within the placebo group a "genuine" effect; that is, relative to their own Threshold I, these subjects took significantly more shocks before announcing pain ($p < .01$) and before stopping ($p < .01$). This "true effect" was not found in Study I, the actual increase for those placebo subjects not being significant, although it *was* significantly greater than the decrease observed for the drug group.

subjects ($p < .02$). These data suggest that although placebo subjects took more shock on the third threshold, they may not have reevaluated the painfulness of these shocks.

## DISCUSSION

The results of Study I and the replication support the hypothesized importance of attribution in generalizing drug-produced behavior changes to the nondrug state. In our analogue to psychoactive drug therapy, subjects were deceived successfully into believing that a drug had significantly raised their pain thresholds and increased their tolerances for electric shock. The crucial attributions were then differentially manipulated by disabusing some subjects (placebo) of the notion that their behavior change could have been caused by the capsule. Deprived of this drug attribution, subjects evidently attributed their improvement to themselves, subsequently maintaining these changes to a greater degree than manifested by control subjects, who continued to attribute their improvement at the second threshold to the drug.[3]

It is important to distinguish our findings from the well-known "placebo effect" (cf. Frank, 1961; Honigfeld, 1964). The placebo effect refers to the phenomenon whereby individuals change with an inactive substance in a direction in which they think they should change. In our own studies above, this would refer to the improvement that might have been observed at Threshold II over and above the surreptitious halving of all shock intensities. This is for our purposes uninteresting; furthermore, conclusions cannot be drawn from our data because improvement from Threshold I to II could be accounted for by adaptation, for which we did not have a control group. Our main results deal with the differences between Thresholds I and III, that is, with the *persistence* of our analogical drug-produced behavioral change on Threshold II. Our disabusing the placebo subjects of the cognition that they had received a true drug may be seen as using a placebo cognition to effect *superior* behavior change, and not as placebos are generally used, namely, as a control for the administration of a drug and the attendant expectations of gain over and above the actual drug effect.

The mechanism by which attribution effected maintenance of the behavior change has not adequately been isolated in this experiment. It was expected that one or more of three processes would be initiated when a subject observed that his changed behavior (on Threshold II) was due to himself: *reevaluation* (the subject would infer that the shocks were not as painful as he originally thought); *changes in expectations and aspirations* (the subject would infer that his subsequent behavior would resemble his behavior on Threshold II and he would be motivated to take more shock); *generation of positive affect* (the subject would be pleased and proud of his improved performance on Threshold II, and these positive feelings would raise his threshold for pain, perhaps by reducing his anxiety). Of these processes, we have data relevant to reevaluation and changes in expectations.

If reevaluation occurred we would expect that prior to the third threshold the placebo subjects would have inferred or convinced themselves that the shocks were really not as painful as they had originally believed. Their performance on the third threshold would then reflect this reevaluation. In accord with this reasoning, placebo subjects did take more shocks on the third threshold before announcing pain and before stopping than they did on the first threshold. Drug subjects did not. Although the changes in pain thresholds were not as strong as the changes in tolerance, both may be considered a reflection of reevaluation (the pain threshold data are understandably weaker when one considers that subjects seemed to find this a difficult judgment, one which in fact results in variances more than twice as large as that of the tolerance threshold). However, if reevaluation did occur, we might also expect that the placebo subjects would have rated the shocks lower in painfulness than the drug subjects when given the opportunity to do so after the third threshold. There was a tendency for this to occur in the first experiment but not in the second.

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[3] Our distinction between self-attributed and drug-attributed behavior change may seem similar in some ways to Rotter's (1966) distinction between internal and external control. We hesitate to stress this apparent similarity, however, inasmuch as Rotter restricts himself to locus of *reinforcement*, whereas it is difficult to find reinforcement operating meaningfully in our paradigm.

It is the absence of strong effects on this measure that leads us to question whether reevaluation of the shocks accounted for the placebo subjects' third threshold performance. Nevertheless, we believe that reevaluation is still a viable explanation. The fact that the subjective pain ratings do not reflect reevaluation might very well be due to their having been obtained *after* the third threshold. At this point in time, placebo subjects might have considered that, although they took more shock on the second threshold than on the first, their third threshold performance was relatively poor. Their inability to match their Threshold II performance might have then led them to infer that the shocks were more painful than they had thought *prior to* the third threshold. In other words, subjects might have evaluated the shocks as less painful after the second threshold and consequently took more shock on the third threshold, *but,* when considering that this last performance was not as good as on the second threshold, they might have inferred that the shocks really were painful. We have here two reevaluations, the second of which may have cancelled the first.

Considering now the subjects' expectations after the second threshold, it will be recalled that in both experiments placebo subjects expected to tolerate more shocks on the third threshold than on the first, whereas drug subjects did not. Although these differences in expectation were predicted, can they alone account for the differences in third threshold performance? If subjects were motivated to match their expectations to take more shock on Threshold III than on I, then behavior on the third threshold could be explained without reference to reevaluation. This is possible and not at all uninteresting. Since we were apparently successful in dissociating the two experiments, subjects would be matching their own expectations and not those of the second experimenter. We believe, however, that this explanation seems to account better for the tolerance data than for the pain data. Tolerance for shock is something that a subject could raise without deceiving the experimenter, whereas, without reevaluation, the recognition of pain could be changed only by deceiving the experimenter. Experimental subjects do not act in this manner! It seems more likely that they would reevaluate the pain of the shocks than harm an innocent experimenter.

Although this study does not permit us to accurately specify the mechanism by which the evaluation of one's behavior affects subsequent behavior, it should be clear that attitude-change research has produced data consistent with ours. Both dissonance research and the data that Bem (1967) has gathered supporting his interpretation of that research lead to the conclusion that attitude change is very much influenced by an individual's explanation for his behavior. If an individual believes that he is responsible for engaging in a behavior which is inconsistent with his own attitude, his attitude will more readily change and become consistent with this behavior than if he believes that external forces are responsible for his behavior. Thus, if there is little external justification for engaging in the behavior (e.g., it is effortful, associated with little reward, not very important to the experimenter or science), the individual is likely to interpret the behavior as self-induced. If it is self-induced the individual can best explain his behavior by forming an attitude that can account for it. Thus, in these situations we find more attitude change than in those situations where there are adequate external justifications for engaging in the behavior.

These data and speculations seem also to have interesting implications for the general area of behavior modification. As an extension of general experimental psychology, behavior therapy is essentially environmentalistic, looking to external variables for the alteration of "abnormal" behavior. The literature indicates to us little concern shown to how the individual so manipulated *perceives the reasons for his changing.* If we make what seems to be the reasonable assumption that the behavior therapy client construes agents of change to be *outside* himself, we have a situation analogous to that dealt with above, namely, a change brought about through external influence. It seems that especially the operant approaches would pose problems for the maintenance of behavior change once the artificially imposed contingencies are withdrawn (cf. Davison, 1969); and the failure thus far to demonstrate *enduring* behavior change via operant procedures might be accounted for at least in part by the notions of attribution proposed here. If a person realizes that his behavior change is totally dependent upon an external reward or punishment, there is no reason for his new behavior to persist once the environmental contingencies change.

Likewise, consider a recent comment (Davison & Valins, 1968) made on the conflicting results obtained with the use of the barbiturate Brevital as a means to induce deep muscle relaxation during systematic desensitization (cf. Brady, 1966, 1967; Friedman, 1966; Reed, 1966). It was suggested that the use of this drug to *impose* relaxation upon an individual might create problems for the generalization of desensitization in imagination to the real life state. It has been observed from experimental studies in desensitization that the transfer of desensitization effects from the imaginal anxiety hierarchy to the life situation is not at all a simple matter (Davison, 1968). The Brevital-desensitization work seems to be a situation identical to the core of this paper: the relaxation produced by Brevital is likely to be perceived by the person as not of his own doing. The important cognition, "I have been able to control my reactions in this situation," seems more probable when persons relax their *own* muscles than when they are relaxed by a drug (cf. Valins & Ray, 1967). This concern is quite separate from the efficacy of Brevital to enable the client to climb his imaginal anxiety hierarchy, just as the experiments reported above do not concern the changes at Threshold II; rather, we would expect that clients who attribute their relaxation during desensitization to a drug rather than to their own efforts will maintain these gains significantly less when confronted with the real situation.

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# EFFECTS OF VOLUNTEERING, FEAR AROUSAL, AND NUMBER OF COMMUNICATIONS ON ATTITUDE CHANGE

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Volunteers and nonvolunteers were randomly assigned to two levels of fear arousal, high and low, and two levels of exposure to the persuasive messages, multiple and single, to complete a 2 × 2 × 2 factorial design. One hundred twenty male undergraduates were used as subjects. The data showed that volunteers complied to a significantly greater extent with the recommendations of the persuasive messages and were affected to a significantly greater degree by the arousal manipulations. The predicted interaction effect on attitude change between volunteers and nonvolunteers was supported. Increasing the number of exposures to the persuasive communication did not affect attitude-change scores.

Rosnow and Rosenthal (1966) reported that a crucial variable in attitude-change studies was whether or not subjects volunteered for the experiment. These investigators reported that volunteers are more amenable to the experimenter's suggestions than are nonvolunteers (Rosnow & Rosenthal, 1966).

Rosenthal (1965), in his review of the literature pertaining to the volunteer subject, reported that volunteers manifest greater intellectual ability, interest, and motivation than do nonvolunteers. Furthermore, volunteers tend to be less authoritarian, less conventional, and have a higher need for social approval than nonvolunteers. Rosenthal (1965) suggested that there is a need for experimentation, on a number of tasks, in which volunteers are recruited and both volunteers and nonvolunteers are used in the experimental design.

The importance of the volunteering dimension is clearly discernible in those studies pertaining to effects of fear arousal on attitude change. Results of experiments which have used nonvolunteers (Janis & Feshback, 1963; Janis & Terwillinger, 1962; Leventhal & Niles, 1964; Singer, 1965; Zemach, 1966) have often indicated a negative relationship between fear arousal and attitude change, though the opposite relationship is sometimes reported (cf. McGuire, 1968).

There is ample evidence, however, that if subjects are solicited on a more voluntary basis high fear arousal will facilitate attitude change. Thus, Leventhal and Singer (1966) reported a positive relationship between fear arousal and attitude change. These investigators conducted their study at an exposition with exhibit-goers who had chosen to enter the particular fair building in which the experiment was being conducted. Dabbs and Leventhal (1966) asked 1,000 subjects to participate in their study and finally recruited 274 subjects. A positive relationship between fear arousal and attitude change was also reported by Leventhal, Watts, and Pagano (1967). These investigators employed volunteers at the moderate fee of $1.50. Similarly, Mann (1967) and Mann and Janis (1968) used volunteers to demonstrate the efficacy of emotional role playing in a higher fear situation.

The evidence suggests that volunteers and nonvolunteers may be differentially affected by high-fear appeals. In this experiment volunteer and nonvolunteer subjects were used in an attempt to delineate the relationship between fear arousal and attitude change. By induction from the previous work, a positive relationship between arousal and attitude change is predicted for volunteers, while, conversely, a negative relationship is predicted for nonvolunteer subjects. Such an interaction would result from enforced participation's aggravating the aggression mechanism suggested by Janis (1968) or the avoidance mechanism posted by McGuire (1968).

A second concern of the present experiment pertains to the observation that attitude-change studies involve, typically, only one experimental session. This is not the usual manner in which we encounter persuasive communications in the mass media. The design of this study will include two levels of exposures, the first presenting five persuasive communications over a period of several days, while the second level will employ the traditional one-exposure approach.

Leventhal and Niles (1965) have reported that attitude change is positively related to duration of exposure to the threat material. While Leventhal and Niles (1965) employed a single exposure, their finding suggests that increasing the number of exposures to the persuasive materials vitiates, to an extent, the immediate resistance (Janis & Terwillinger, 1962) to the high-fear communication. It is expected, therefore, that multiple exposures facilitate acceptance of the communications' recommendations to a greater extent than a single exposure to the message. A second prediction is that under low levels of arousal where initial resistance should be negligible, multiple exposures will manifest a cumulative effect and hence elicit greater attitude change than would a single exposure.

## METHOD

### *Subjects and Design*

The subjects were 120 male students enrolled in the basic psychology course at the University of Toledo. Sixty subjects were nonvolunteers, recruited by selecting their names from a subject file-card index and requiring that they appear at the laboratory at the appointed time. These subjects received experimental credit for their participation. The other 60 subjects responded to a request to participate in a psychological experiment. These subjects were also enrollees in the basic psychology course.

The two types of subjects, volunteers and nonvolunteers, two levels of arousal, high and low, and two levels of exposures, multiple and single, were combined in a $2 \times 2 \times 2$ factorial design. Each cell contained an $n$ of 15.

### *Procedure*

Experimental sessions were conducted with groups ranging from three to six subjects. Volunteers and nonvolunteers were randomly assigned to arousal and exposure conditions. The male experimenter was not informed as to whether a particular group of subjects were volunteers or nonvolunteers. To reduce further the subjects' tendencies to conform to demand characteristics, they were informed that their responses would remain anonymous in the experiment (Silverman, 1968).

### *Communications*

The communications were presented in separate eight-page pamphlets which were entitled, "The Uses and Effects of Drugs." All communications contained passages on the recourses citizens had for controlling and meliorating the deleterious effects of drug abuse. Each communication offered three basic recommendations for eliminating these drug abuses. These suggestions pertained to greater control of and increased ethical standards in drug research; increased control over the labeling and advertising of drugs; and more careful prescription and distributions of harmful drugs.

### *Arousal*

High-fear-arousal materials emphasized the dangerous consequences of drug misuse and abuse. Case histories and vivid descriptions of these effects were employed. The information and descriptions for these communications were supplied by materials published by the United States Department of Health, Education, and Welfare.

Some of the pamphlet topics were drug catastrophes, especially the thalidomide case; the indicated relationship of oral birth control pills and thrombophlebitis and pulmonary embolisms; the dangers of amphetamines and barbiturates; the consequences of taking hallucinogenic drugs, especially LSD; and the sometimes fatal or near-fatal reactions to aspirin, penicillin, and other less exotic drugs.

Subjects in the high-arousal condition were also shown two films. One was entitled "The Mind Bender," which explored the uses and hazards of LSD and other hallucinogens. The second film pertained to amphetamines and barbiturates, and was based on interviews with actual victims of "pill-popping." Both films were releases of the United States Public Health Service.

The low-fear materials were concerned with the same topics and differed from the high-fear materials only to the extent that vivid verbal descriptions of death and disability resulting from drug abuse were excluded. The incidence of death and disability, however, was included. The low-fear materials were written with the intent of conveying, as forcefully and convincingly as the high-fear materials, the dangers inherent in drug abuse. Subjects in the low-fear condition were not shown the two films.

### *Exposures to Communications*

Subjects in the multiple-exposure condition were exposed to five separate messages within the space of 3–5 days. In addition the high-fear-arousal group saw both films during this space of time. The messages and the films were presented randomly. The single-exposure group participated in one session,

TABLE 1

MEAN ATTITUDE CHANGE SCORES AND STANDARD DEVIATIONS FOR THE EIGHT EXPERIMENTAL CONDITIONS

| Condition | Volunteers | | | | Nonvolunteers | | | |
|---|---|---|---|---|---|---|---|---|
| | High fear | | Low fear | | High fear | | Low fear | |
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Multiple exposures | $8.13_a$ | 1.52 | $6.26_{abc}$ | 2.26 | $4.13_d$ | 2.21 | $5.60_{bcd}$ | 2.06 |
| Single exposure | $7.07_{ab}$ | 1.69 | $5.33_{bcd}$ | 1.01 | $4.46_{cd}$ | 1.96 | $4.86_{cd}$ | 2.33 |

Note.—Means with common subscripts are not significantly different. $n = 15$ in each group.

and was exposed to a single message. The communication was randomly chosen. In addition, the high-fear-arousal group viewed one of the two films, again randomly presented.

### *Dependent Variables*

A postexperimental questionnaire was administered to the subjects on which they indicated, on a 10-point scale ranging from "it did not affect me at all" to "it made me greately concerned and upset," their reaction to the communication. Thus, for the single-exposure group, there is one general arousal score, and five such scores for the multiple-exposure group. Subjects also responded to a second 10-point scale indicating the perceived degree of coercion used in the experimental manipulations. The amount of attitude change was obtained by having the subjects respond to three 10-point scales corresponding to each of the three basic recommendations. General attitude-change scores were arrived at by averaging the three responses for each subject.

## RESULTS AND DISCUSSION

The effect of the experimental manipulations on attitude change is presented in Table 1. The data clearly indicate that volunteering is a crucial variable in fear-arousal experiments. Volunteers manifest greater attitude change than do nonvolunteers. Examination of the means reveals the predicted positive relationship between fear arousal and attitude change for volunteers and the predicted negative relationship for nonvolunteers. The Volunteer × Fear interaction is significant.

An analysis of variance performed on the self-reported arousal scores revealed that volunteers were significantly more affected by the arousal manipulations than were nonvolunteers ($F = 15.4$, $df = 1/112$, $p < .01$). The analysis also revealed that the fear manipulation was successful ($F = 23.5$, $df = 1/112, p < .01$).

The implications of the self-reported arousal scores are difficult to determine. It is possible that the scale measuring degree of arousal could have been interpreted by the subjects as a measure of the persuasiveness of the messages used. Therefore, perceived arousal and message persuasiveness could very well be confounded. To complicate interpretation of these data further, Lazarus and Alfert (1964) have reported an oppositive relationship between self-report scores and arousal as indicated by autonomic measures for subjects categorized as high deniers.

An analysis of variance performed on subjects' perceived degree of coercion scores revealed that volunteers felt significantly less coerced than did nonvolunteers ($F = 6.97$, $df = 1/112$, $p < .01$). An internal analysis of the perceived degree of coercion scores was accomplished by the use of a median test. This analysis indicated that the less coerced volunteers felt, the greater was their attitude change ($\chi^2 = 4.75$, $df = 1$, $p < .05$). Felt coercion, however, did not affect the attitude-change scores of nonvolunteers ($\chi^2 < 1, df = 1$).

TABLE 2

ANALYSIS OF VARIANCE

| Source | *MS* | *F* |
|---|---|---|
| Volunteers (V) | 112.13 | 28.38* |
| Fear (F) | 12.03 | 3.04 |
| Exposures (E) | 10.73 | 2.71 |
| V × F | 40.83 | 10.33* |
| V × E | 4.80 | 1.21 |
| F × E | 5.63 | 1.42 |
| V × F × E | 11.54 | 2.92 |
| Error | 3.95 | |

* $p < .01$, $df = 1/112$.

The importance of subjects' felt coercion has not been clearly delineated by the present study. The design of the present study does not permit a definite conclusion as to whether felt coercion differences were due to the initial recruitment of different subject population or due to the experimental manipulation. This could be remedied by further research which would hold degree of coercion constant while manipulating volunteering within the same sample; and by manipulating coercion while holding the vounteering dimension constant.

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# MACHIAVELLI AND THE DEVIL'S ADVOCATE[1]

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High and low scorers on Christie's scales measuring agreement with statements by Machiavelli were subjects in an experiment contrasting counterattitudinal role playing with a non-role-playing condition. Role-playing subjects were required to improvise a talk against fluoridation. Monetary incentive was kept low and constant across conditions. On the basis of a hypothesized difference in susceptibility to dissonance stemming from discrepant role playing, it was predicted that opinion-change scores would display an interaction effect between Machiavelli scale score and role-playing condition in the direction of greater opinion change by low Mach scorers in the counterattitudinal role-playing condition. The prediction was sustained.

Previous studies have dealt with the persuasibility of high versus low scorers on Christie's (1962) scales measuring agreement with statements by Machiavelli (Geis, Krupat, & Berger, 1966; Jones, Gergen, & Davis, 1962). The findings have generally supported the expectation that high Mach scorers—that is, those holding Machiavellian attitudes—are less persuasible than low Mach scorers in a number of situations.

At the same time, numerous studies, dating from Kelman (1953) and Janis and King (1954), have repeatedly reported opinion change following discrepant role playing. However, no previous role-playing experiment had successfully predicted *differential* opinion change on the basis of subjects' measured attitudinal or personality characteristics.

The original association of ideas leading to this study was a somewhat intuitive one in which counterattitudinal role playing was thought of as "playing devil's advocate"—and "playing devil's advocate" was thought of as a singularly apt exercise for high-scoring Machiavellians. Certainly, the stereotype of a Machiavellian encompasses the idea of a willing dissimulator, role player, or manipulator, a person to whom the truth is less important than the immediate point he is making or the impression he is trying to give. On the other hand, a low scorer on these scales should be an individual for whom dissimulation of his true feelings or suppression of the truth would be utterly dissonant with his values and his self-image.

Thus the thought of placing high and low Mach scorers in a counterattitudinal role-playing experiment led to the specific prediction that opinion-change scores would show an interaction effect between Mach score and dissonance condition—that is, discrepant role-playing versus non-role-playing condition—in the direction of greater opinion change by *low* Mach scorers in the *role-playing* condition.

## METHOD

The subjects were 80 male undergraduates at Columbia University drawn from social science classes to which had previously been administered the two Machiavelli scales—Mach IV, a Likert-type form, and Mach V, a forced-choice form—and also a Likert-type measure of the students' opinions on a number of currently controversial topics. All students placed in the pool from which subjects were drawn were Americans under 30 years of age who met the criteria set by Christie for being either a low Mach or a high Mach scorer in this sample on both forms of the Machiavelli scale.[2] In addition, each student in the pool had indicated a position *in favor of fluoridation* on the survey opinion questionnaire.[3]

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[1] This paper is a partial summary of a dissertation submitted to the Department of Social Psychology, Columbia University in partial fulfillment of the requirements for the PhD degree. A paper reporting these data was read at the Annual Meeting of the Eastern Psychological Association in Washington, D. C., April 20, 1968.

[2] The formula for combined total Mach scores used was based on a median split of the scores made on each form of the Machiavelli scale by the Columbia classes to which it was administered in the fall of 1965. The medians fell between 81 and 82 on the Mach IV scale and between 14 and 15 on the Mach V scale. The formula is described in Christie's (1964) paper.

[3] The initial opinion means on fluoridation for high and low Mach scorers who actually took part in the experiment proved to be almost identical—

After the sample pool had been established, 40 high Mach and 40 low Mach scorers were recruited and assigned by a random procedure to either the role-playing or non-role-playing conditions. An additional manipulation, intended to add positive versus negative sponsorship to the variables, was attempted by telling half of the subjects within each group that the sponsor of the radio series was the American Association for the Advancement of Science, while the other half of each group was told the sponsor was the National Committee Against Fluoridation. This secondary manipulation, although statistically significant, did not produce a truly polarizing effect, since both sponsors were perceived somewhat positively. Therefore, data will be reported solely in terms of the four major groups—namely, (*a*) high Mach role players, (*b*) high Mach non-role players, (*c*) low Mach role players, and (*d*) low Mach non-role players.

Individual subjects were recruited by telephone, ostensibly by the representative of an educational radio service interested in obtaining participation by college students in a planned radio discussion series dealing with public affairs. No further details as to specific task or topic were given but, as an inducement to take part, all subjects were told that the company would pay a nominal fee of $2 to each student participant. Monetary inducement was thus held constant for all conditions at what was deemed to be a small but credible amount for an outside organization to offer.

This approach proved successful in securing participation even from subjects who, in class, had indicated unwillingness to take part in experiments. Thus the participants appear to have viewed the situation as a real one, not as an experimental study. Of the 94 students contacted by telephone, 84 actually appeared for interviews. Of these, 2 refused to continue at the point when they were asked to improvise a talk against fluoridation. Of the 82 who completed the session, 2 interviews contained errors or omissions in procedure, and data for 80 have been reported.

### *Experimental Procedure*

When a subject appeared for his interview, he was met by the experimenter who explained that the company was currently working on a tape-recorded discussion series which would subsequently be sold to small radio stations, including college stations, all over the country. The experimenter then spoke approximately as follows:

> The issue we're working on just now is the question of fluoridation of public water supplies. As you may know, this is still a hot issue in some parts of the country. Now, we've already completed a tape giving views in favor of fluoridation—and now we're working on the show giving views against fluoridation.

At this point the experimenter handed the subject either Booklet A or Booklet B, depending upon whether the subject had been assigned to the "positive sponsor" or the "negative sponsor" treatment. Booklets A and B differed only in the designated sponsor for the series. The arguments contained in them were exactly the same. The experimenter then proceeded to take *role-playing* subjects through the following sequence of steps: (*a*) The subject read the brief, one-page statement of arguments against fluoridation; (*b*) the subject received the $2 participation fee; (*c*) the subject was asked to improvise a talk against fluoridation on the spot; (*d*) the subject made his improvised speech, speaking into a tape recorder and identifying himself by name and school affiliation at the start of the talk; (*e*) the final questionnaire measure of opinion change, liking for task, self-ratings of performance, and checks on manipulations was administered. *Non-role-playing* subjects followed the same first two steps in the sequence—that is, they read the same antifluoridation arguments and received the same participation fee. Immediately after these two steps the final questionnaire measure of opinion change was taken, and this concluded the non-role-playing condition.

## RESULTS

Opinion change was measured by responses to a criterion item on the issue of fluoridation presented before and after treatment. Responses were made on an 11-point rating scale, and opinion-change scores refer to actual amount of movement on this scale. Since experimental treatment attempted to influence profluoridation subjects in an antifluoridation direction, change in the antifluoridation direction was scored as positive. Only one subject in the role-playing condition and no subject in the non-role-playing condition had a negative change score.

The major hypothesis had predicted that opinion-change scores would show an interaction effect between high versus low Mach score and role-playing versus non-role-playing conditions, in the direction of greater opinion change by low Mach scorers in the role-playing condition. A 2 × 2 × 2 analysis of variance of opinion-change scores is shown in Table 1. The interaction between Mach score and role-playing condition was significant in the predicted direction ($p < .025$).

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9.625 for high Mach scorers and 9.675 for low Mach scorers—although no attempt to match subjects on initial opinion position had been made, beyond the requirement that the sample pool was restricted to students initially favorable towards fluoridation, that is, those having checked any position on the favorable side of the neutral point on an 11-point rating scale.

TABLE 1

ANALYSIS OF VARIANCE OF OPINION CHANGE SCORES

| Source of variation | *SS* | *df* | *MS* | *F* |
|---|---|---|---|---|
| Machiavelli score (A) | .80 | 1 | .80 | — |
| Role-playing condition (B) | 5.00 | 1 | 5.00 | 1.06 |
| Sponsorship (C) | 1.80 | 1 | 1.80 | — |
| A × B | 26.45 | 1 | 26.45 | 5.60* |
| A × C | .05 | 1 | .05 | — |
| B × C | 4.05 | 1 | 4.05 | — |
| A × B × C | 3.20 | 1 | 3.20 | — |
| Error variance | 340.20 | 72 | 4.725 | |
| Total | 381.55 | 79 | | |

* $p < .025$.

Table 2 shows the mean opinion-change scores for the four groups. The major hypothesis of the study was confirmed by the data: high and low Mach scorers did manifest *differential* opinion change in the role-playing and non-role-playing conditions.

The final questionnaire instrument included ratings of liking for task and self-ratings of the effectiveness of the improvised talks. Contrary to predictions, no significant differences between high and low Mach scorers were found on either liking for task or on self-ratings of effectiveness.

In addition, the improvised speeches which had been tape-recorded were rated for effectiveness by two independent judges who were told only that the speeches had been made to see how well college students could improvise talks. Thus the judges were not aware of the experiment. Each judge rated each speech for overall effectiveness on a 6-point scale. Interjudge agreement was high. In fact, in only two cases were there disagreements of even as much as 2 points on the scale. The judges' two independent ratings were averaged together to form a composite rating of overall effectiveness for each speech. A complete analysis of variance of these composite ratings showed no significant differences between groups.

TABLE 2

MEAN OPINION CHANGE SCORES

| Condition | High Mach scorers | Low Mach scorers |
|---|---|---|
| Role playing | 1.50 | 2.85 |
| Non-role playing | 2.15 | 1.20 |

Note.—$N = 80$.

## DISCUSSION

The central finding of the present study was that differences in opinion change following discrepant role playing could be predicted from the Machiavelli scale scores of subjects. The fact that this prediction referred to an interaction effect means that the present study moves in the direction of indicating a specific condition under which low Mach scorers proved to be more persuasible than high Mach scorers, and a different condition under which the reverse was true.

The finding that high Mach scorers changed more than low scorers in the non-role-playing condition appears, at first sight, to contradict the findings from previous studies cited (Geis et al., 1966; Jones et al., 1962). However, closer examination shows that, in each of those studies, greater opinion change by low Machiavellians stemmed from experimental conditions involving some form of social influence. Such experimental conditions are not completely comparable to the non-role-playing condition in the present study. Instead of a contradiction in results, there is rather an indication that the relative persuasibility of high and low Machiavellians may differ when seen in social versus nonsocial contexts.

Specifically, the present study found that high Machiavellians changed more than low Machiavellians when the (non-role-playing) treatment consisted of reading "factual" arguments against fluoridation. This suggests that high Mach scorers may be more susceptible to persuasion by seemingly "factual" arguments than low Mach scorers. A study by Harris (1966) supports this particular finding. Harris exposed high and low Mach scorers to "factual" type versus "bandwagon" type counterattitudinal arguments. His findings were that low Mach scorers were more influenced by the "bandwagon" type of argument (analogous to a social influence condition), while high Mach scorers were more influenced by "factual" arguments, although the interaction was not significant in his study. Harris' "factual arguments" condition was analogous to the non-role-playing condition in the present study and the results were similar in direction.

The present finding of an interaction effect between Mach score and role-playing condition points to a relationship between the nature of the task and the dynamic differences between high and low Machiavellians. It thus points up the need for investigation into what might be called the "internal dynamics," represented by a high or low score on the Machiavelli scales. To date, these internal dynamics have remained largely unexplored.

Previous studies have concentrated for the most part on the comparative performances of high and low Mach scorers at tasks requiring manipulation of others. For example, in a study by Geis et al. (1963), subjects were placed in a face-to-face situation with a peer and given considerable leeway in selecting means of "conning" the peer by administering false feedback on test scores or by distracting the peer in various ways from his task. High Mach scorers in that study reported significantly greater liking for task than did low Mach scorers. The present experimental situation, by contrast, was one in which the subject never came face to face with his "audience" and knew that he would have no opportunity to observe the effect his talk might have. An interpretation suggesting that the opportunity to exercise power over visible others was the source of differential task liking in the Geis et al. (1963) study would therefore be consistent with the finding of no such differences in liking for task in the present study.

The importance of differences in the nature of the role-playing task itself has been demonstrated by Carlsmith, Collins, and Helmreich (1966), who found that a "face-to-face" role-playing condition led to results consistent with a dissonance prediction for their experimental situation, while an "anonymous essay writing" condition led to results consistent with an incentive prediction. It is at least plausible that such differences in the role-playing task may have differential effects for high and low Mach scorers.

Another line of inquiry that seems indicated would be the investigation of possible relationships between Machiavelli scores and other attributes related to attitude change following role playing—for example, Elms (1966) found evidence of a relationship between empathic fantasy abilities and opinion change following discrepant role playing.

Finally, since one implication of the present study suggests that low Mach scorers are characterized by sincere involvement in their own beliefs and actions, while high Mach scorers are characterized by greater detachment, it would appear that further research with high and low Machiavellians, involving manipulation of belief commitment and incentive, might prove fruitful.

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# ENDORSEMENT OF THE GROUP SPOKESMAN AS A FUNCTION OF HIS SOURCE OF AUTHORITY, COMPETENCE, AND SUCCESS[1]

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An experiment was conducted to investigate the effects of three factors legitimizing the leader's position, that is, his initially perceived competence, his source of authority in election or appointment, and his task success. One hundred thirty-six male students participated in four-person discussion groups, arrayed in a 2 × 2 × 2 factorial design. Each group developed a defense for a fictitious friend supposed to have been accused of cheating; thereafter, a group "spokesman" was designated ostensibly to present the defense before a board of inquiry. The choice of a spokesman, by election or appointment and by level of perceived competence, as well as his subsequent success, were controlled by the experimenter. As hypothesized, reactions to the spokesman differed as a function of all three factors. The more competent and successful spokesman was endorsed more strongly than the less competent and unsuccessful spokesman; the election or appointment of the spokesman yielded complex interactions with competence and success in determining the favorability of the members' reactions. Implications are drawn from these results regarding the significance of legitimizing mechanisms in shaping group members' reactions to the spokesman role in leadership.

How a leader attains and maintains his position is among the most important aspects of leadership. This question of "legitimacy" has largely been considered a matter of theoretical significance, however, rather than a basis for empirical work. Though a distinction between legitimate and illegitimate authority has received some attention (see French & Raven, 1959; Raven & French, 1958a, 1958b), virtually no systematic attempt has been made to uncover the implications of different bases of legitimate authority.

In the present investigation the effects of three factors legitimizing the leader's position are studied: his perceived competence, in terms of task capability; the mechanism by which he attains his position of authority, that is, either election or appointment; and his subsequent task success. The focus here is on group members' favorability toward the leader in his role as spokesman for the group. In general terms, the importance of positive endorsement of the leader by followers has been recognized in a number of recent conceptualizations of leadership (e.g., Fiedler, 1967; French & Raven, 1959; Hollander & Julian, 1968; Janda, 1960). A prevalent theme in these treatments is the specification of the followers' perceptions of the leader which form the basis for their acceptance of his influence. For example, Janda has identified leadership as a type of legitimate power relationship, in which the power wielder is perceived as having particular rights with reference to the activities of the power recipients. In a related vein, Hollander and Julian (1968) have emphasized the leader as a group member perceived to provide a necessary resource for the accomplishment of group goals.

For this study, endorsement is conceived as a commitment by group members to the continued incumbency of the leader in his role. Endorsement therefore implies continuity of support for the leader, and a willingness to accept his influence. As such, it may be taken as an index of the viability of the leader-follower relationship. The present investigation is predicated on the view that different mechanisms which legitimize the lead-

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er's authority have differential impacts on the expectations about, and endorsement of, the leader. This rationale was explored recently by Evan and Zelditch (1961) in their study of bureaucratic authority, where they pointed out the potential interaction of the two dimensions of Weber's bureaucratic authority: the authority of knowledge and the authority of office. In that investigation, reactions of group workers to a legitimate (appointed) supervisor were strongly influenced by his knowledge or expertise in the job. Their study, however, is one of only a few which shed light on effects of source of authority. Such related efforts are seen in the work by Carter, Haythorn, Shriver, and Lanzetta (1951), Cohen and Bennis (1961), Goldman and Fraas (1965), and Hollander, Julian, and Perry (1966). While the results of these studies confirm the importance of the source of the leader's authority, they do not provide the basis needed to examine the relationship between source of authority and member endorsement of the leader.

The effects of two general procedures are investigated in the present study, that is, election and appointment. Though both are legitimizing, they can be seen to involve important differences. Intuitively, the degree of involvement and shared responsibility is greater for the group member who elects his leader. On the other hand, the appointed leader may be less vulnerable to group pressures. Our general expectation is an interaction between source of authority and the perceived competence or success of the leader in determining member support.

A second major determinant of group support for the leader is the leader's ability to contribute to the achievement of group goals. Other things equal, more competent leaders are found to be more strongly endorsed by the group. Investigations by Evan and Zelditch (1961), Hamblin, Miller, and Wiggins (1961), Hollander (1960), Hollander et al. (1966), Julian and Hollander (1966), and Maier (1950) have, in various ways, tended to support this generalization. There are, however, at least two factors which can lead to the perception of the leader as one who can make positive contributions to group achievement: his perceived competence, with respect to the task itself, and his success in delivering a favorable outcome. Although these two factors are highly interdependent, the former builds the expectation that the leader will contribute to the accomplishment of group goals and the latter provides a degree of confirmation or disconfirmation of this expectation. Hence, our prediction is an interaction between the leader's perceived competence and his task success, with the level of task success anticipated to have a greater effect on the endorsement of leaders initially seen as competent than on those seen as not competent.

In sum, it is hypothesized that election and appointment will differentially affect the favorable endorsement of the leader in combination with his perceived competence or success. The appointment condition is expected to yield favorable reactions only where the leader is seen to be competent or successful. As in past findings, it is anticipated, overall, that leaders perceived to have value for the group (competent and/or successful) will be favored over those not having such value, and that this effect will be most marked when success follows perceived competence.

## METHOD

A task was created in which groups of students were given the responsibility for developing a defense for a hypothetical friend who was accused o cheating. After a short discussion, the group allegedly was to send a spokesman before a board o inquiry which was investigating the case. The spokesman's job was to persuade the board of the friend's innocence. Within this setting, the spokesman's source of authority, perceived competence, and task success were manipulated.

### *Subjects and Procedure*

One hundred thirty-six volunteer freshmen and sophomores from the Air Force ROTC program a State University of New York at Buffalo participated as the members of four-person discussion groups. A their assigned hour, an average of 20 men reported to a central screening point where they were sen to individual rooms, 4 to a room. Room assignments were made in a random sequence in an effort t separate friends who had reported together.

After the men arrived at the room they wer greeted by an experimenter, introduced to one another, and given an initial questionnaire to complete. After all members had completed the questionnaire, the discussion began with a review of th cts" of a fictitious case of cheating on an exam a supposed friend of theirs. Groups were told t as part of the experiment, "Roger's case" was be presented before a board of inquiry comprised fellow ROTC cadets and that their immediate was to develop arguments for his defense. Each cussion lasted approximately 20 minutes. At the d of the discussion, the experimenter suggested t it was time to choose a spokesman who would resent the group before the board in Roger's be- lf. At this point, the experimenter arranged the ocedure so as to manipulate the apparent source of thority and perceived competence of the okesman.

### *anipulations and Inductions*

In order to manipulate the perceived competence the spokesman, the experimenter-observer for each oup made tallies during the discussion of the fre- ency of participation of each member. The most equent contributor to the discussion was chosen as e spokesman in the "high perceived competence" ndition, while in the "low perceived competence" ndition the member who ranked next to lowest in rticipation was chosen spokesman. The intention as simply to identify individual group members ho were perceived by their peers as more or less le to perform well as the spokesman for the oup. Regula and Julian (1967) have recently plicated the usual finding that frequency of par- cipation dominates such perceptions.

As the manipulation of source of authority, the okesman was either chosen by a mock election or y the experimenter's appointing him. Under the ection condition ballots were secret, and members nk ordered their choices, with the winner being nnounced after an ostensible calculation of the ighest average rank. Appointment of the spokes- an was made arbitrarily by the experimenter after n obvious inspection of the notations he had made uring the discussion.

The manipulation of the spokesman's success or ilure was accomplished on a random basis across roups. Verdicts of "acquitted" or of "guilty," carry- g the penalty of failure in the course, were handed the spokesman, and this constituted the sole basis or the group's knowledge of his success or failure.

After the spokesman was chosen, he was asked to eport to another room where the inquiry was to ake place. The spokesman was allowed to bring ny notes or information from the group's discussion hich he thought would be helpful. When the spokes- an had left, the experimenter distributed Question- aire 1 asking for evaluations of the spokesman and he discussion. These ratings provided a check on he manipulation of perceived competence. After a 0–15-minute delay the spokesman returned to his roup with a sealed envelope containing the board's erdict. After opening the envelope and reading the erdict aloud, the experimenter asked the spokesman gain to step outside the room while the group valuated the verdict. Group members then gave atings on Questionnaire 2 evaluating the spokesman's performance in the context of the verdict. It was at this point that members signified the extent to which they endorsed the spokesman.

Each group was then adjourned and everyone returned with the spokesman to a central room for debriefing. Group members were told by the project director that it had not been possible to complete the experiment because of limited personnel; that there had been no board of inquiry; therefore, the verdicts which their groups had received were assigned on a purely random basis, and perhaps the investigation might be completed on a future occasion. The men were then given the chance to volunteer for additional sessions.

There were several indications of the interest which this task elicited from the subjects. Two thirds of the men indicated their interest in participating again and specified times when they would be available. In addition, the experimenters rated these sessions as relatively friendly, pleasant, relaxed, interesting, and productive, while informally they reported the spokesmen to be tense as they left the room to go before the board, taking care to preserve any notes that they had made. Later, the spokesmen were audibly pleased when told that it would not be possible for them to present their defense of "Roger." We were therefore encouraged to believe that the situation was mildly involving, at the very least, and that the subjects were actually concerned about task outcomes.

### *Variables and Measures*

The independent variables of source of authority and perceived competence formed first a 2 × 2 factorial design for the analysis of Questionnaire 1, and then, with the addition of the spokesman's success or failure, these variables formed a 2 × 2 × 2 design for the analysis of Questionnaire 2. Since there were slight discrepancies in the number of groups per cell, an unweighted-means analysis was used. All scores were group averages of individual member responses.

The major dependent measures were ratings completed by the discussants immediately following the discussion (Questionnaire 1) and then following the announcement of the verdict (Questionnaire 2). These questionnaires were comprised of 6-point graphic rating items which will be described in the discussion of the results. Endorsement of the group spokesman was assessed in Questionnaire 2 by asking: How willing would you be to have the spokesman represent the group again (Table 1, item 6)?

Members also completed a semantic differential description of the spokesman as part of Questionnaire 1 which was factor analyzed to determine the clustering of scales. Two factors emerged: an "evaluative" factor, which was composed of the scales friendly-unfriendly, fast-slow, active-passive, pleasant-unpleasant, strong-weak, confident-unsure, and dependable-undependable; and a second factor which was defined by only two scales, warm-cold and

TABLE 1

MEAN RATINGS OF GROUP SPOKESMAN

| Variable | Perceived competence | | | |
|---|---|---|---|---|
| | Elected | | Appointed | |
| | High ($n = 9$) | Low ($n = 9$) | High ($n = 8$) | Low ($n = 8$) |
| 1. How much did spokesman contribute to discussion? | 5.1 | 3.7 | 4.7 | 3.1 |
| 2. How well qualified is spokesman? | 4.4 | 3.7 | 4.0 | 3.3 |
| 3. How satisfied with choice of spokesman? | 4.6 | 3.8 | 4.4 | 3.7 |
| 4. Spokesman description: evaluative dimension (low = positive) | 16.0 | 19.9 | 17.8 | 20.0 |
| 5. Spokesman description: emotional acceptance dimension (low = positive) | 7.9 | 8.4 | 7.9 | 7.2 |
| 6. How willing would you be to have spokesman represent group again?[a] | 4.9 | 4.2 | 5.1 | 4.4 |

[a] This item was a part of Questionnaire 2 given at the end of the experiment; all others were included in Questionnaire 1.

soft-hard, appearing to reflect an "emotional acceptance" dimension.

## RESULTS

### *Endorsement of the Spokesman*

The major findings concerning group member endorsement of the spokesman are presented in Table 1 and Figure 1. A summary of the corresponding analysis of variance is shown in Table 2. Though there were no overall differences in the endorsement of elected as against appointed spokesmen, as predicted, group members gave a significantly stronger endorsement to the spokesman seen as more competent. These subjects were more willing to have him represent the group again (Variable 6, Table 1; $F = 11.24$, $df = 1/26$, $p < .01$), and better satisfied with him as spokesman (Variable 3; $F = 4.13$, $df = 1/30$, $p < .05$).

TABLE 2

SUMMARY ANALYSIS OF VARIANCE: ENDORSEMENT OF GROUP SPOKESMAN

| Source | *MS* | *F* |
|---|---|---|
| Source of authority (A) | 1.03 | 1.19 |
| Perceived competence (B) | 9.68 | 11.24** |
| Success (C) | 6.36 | 7.39* |
| A × B | .02 | <1.00 |
| A × C | 1.28 | 1.49 |
| B × C | .01 | <1.00 |
| A × B × C | 4.70 | 5.46* |
| Error (within) | .86 | |

* $p < .05$, $df = 1/26$.
** $p < .01$, $df = 1/26$.

The spokesman's success in representing the group also influenced member endorsement; endorsement of the successful spokesman was 4.9 and the unsuccessful, 4.3 ($F = 7.39$, $df = 1/26$, $p < .05$). The predicted interactive effect of perceived competence and success on endorsement did not occur, however, nor were there the predicted second-order interactions of source of authority with perceived competence and success, respectively.

The importance of source of authority for endorsement did emerge, however, in the triple interaction of perceived competence, success, and authority source shown in Figure 1 ($F = 5.46$, $df = 1/26$, $p < .05$). Although the level of endorsement had not differed for the elected and appointed spokesman, the nature of the interaction between success and perceived competence took quite a different form for the elected and appointed conditions. For the elected spokesman, an incompetent man was rejected regardless of his success at the task, whereas success qualified the endorsement of a competent man. For the appointed spokesman, however, it was the competent man who was relatively immune to the effects of success or failure, while his incompetent counterpart suffered a dramatically lowered endorsement if he failed.

### *Perceptions of the Spokesman*

Perceptions of the spokesman varied as function of both source of authority and hi perceived competence. Although difference

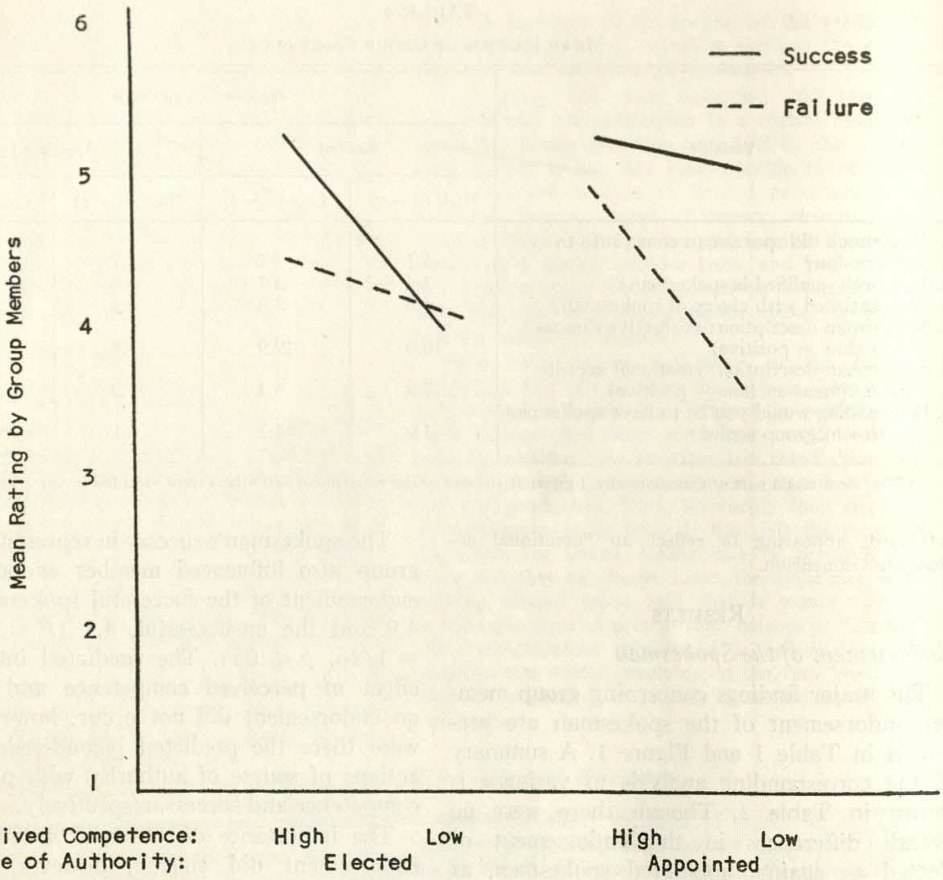

FIG. 1. Mean member willingness to have spokesman represent group again, presented for each condition.

due to source of authority were generally nonsignificant, there was a tendency to evaluate the *elected* spokesman in more positive terms than the appointed man. He was judged as having contributed more ($F = 2.88$, $df = 1/30$, $p < .10$), as being better qualified ($F = 3.99$, $df = 1/30$, $p < .10$), and as more "accepting" ($F = 4.42$, $df = 1/30$, $p < .05$). There were no trends for Variables 3 and 4.

In addition, these ratings of the spokesman clearly confirmed the manipulation of his perceived competence. Variable 1 was most similar to the criterion of competence used for the selection of the spokesman: "How much did the spokesman contribute to the discussion?" Here, differences as a function of the perceived competence conditions were highly significant, with the more talkative spokesman rated as having contributed more to the discussion (4.9 and 3.5 for high and low perceived competence, respectively; $F = 40.1$, $df = 1/30$, $p < .01$). Consistent with this result were the ratings of the perceived qualification of the spokesman ($F = 8.18$, $df = 1/30$, $p < .01$), and the members' general evaluation of the spokesman ($F = 5.9$, $df = 1/30$, $p < .05$). Hence, it is encouraging that the criterion of perceived competence did fit the group members' perceptions rather well.

### *Satisfaction with the Verdict*

Success was defined as a verdict of acquitted, while failure was a verdict of guilty. The appropriateness of these definitions was evident in a final questionnaire item which asked: "How satisfied were you with the outcome?" Satisfaction with the acquittal verdict was rated 5.2, with the guilty verdict, 2.1 ($F = 123.1$, $df = 1/26$, $p < .01$). A com-

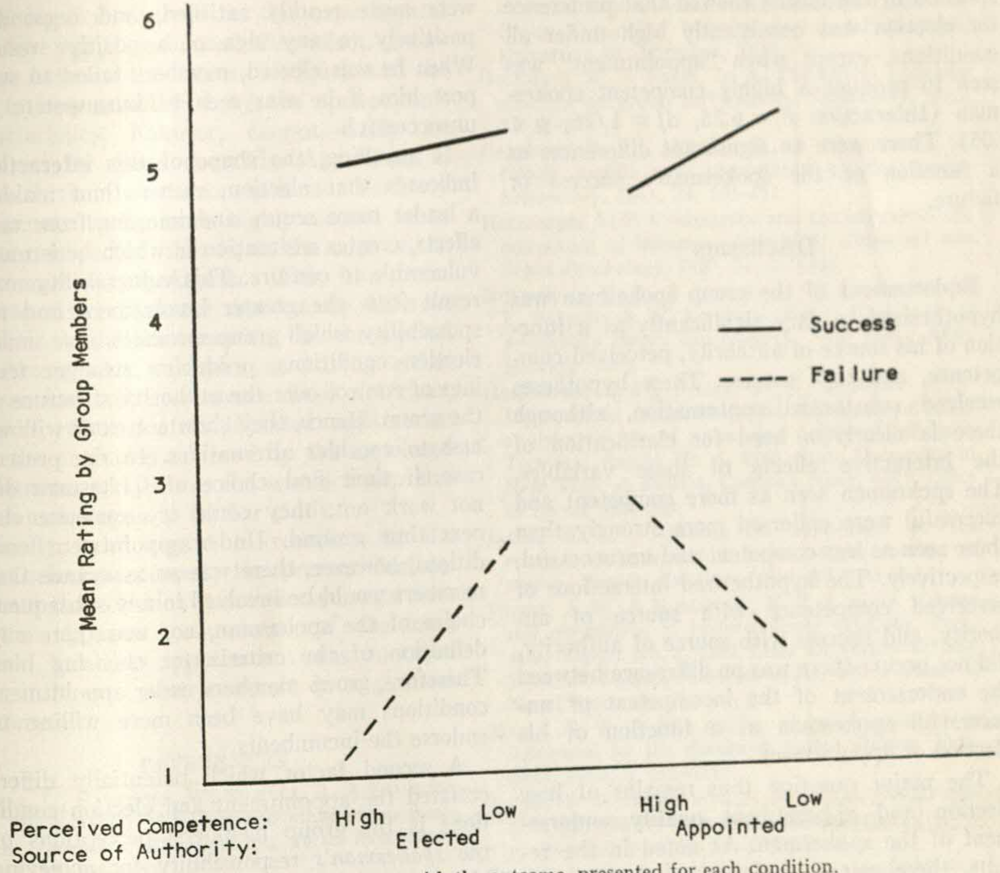

FIG. 2. Mean satisfaction with the outcome, presented for each condition.

plex interaction, shown in Figure 2 ($F = 5.77$, $df = 1/26$, $p < .05$), also indicated that group members were well satisfied with a successful outcome, regardless of the spokesman's perceived competence or source of authority. Satisfaction following failure, however, differed significantly and in a diametrically opposite fashion for the elected and appointed spokesman under the high-perceived-competence condition. It is clear that the spokesman's failure had a quite different meaning depending upon the expectations established by his perceived competence and source of authority.

*Reactions to Election and Appointment*

In addition to the endorsement of the group spokesman, the final questionnaire included two items to assess member evaluations of the leader selection process. Although no predictions had been made, we were guided by the notion that endorsement of the selection procedure would be influenced by "how well it appeared to work." Hence, judgments were expected to vary as a function of both competence and success. A "preference for election" was derived as the difference between responses to the two questions which asked: "To what extent would you approve having the group elect the spokesman if you were to meet to discuss additional cases?" and "To what extent would you approve having the spokesman appointed, etc.?"

Parenthetically, it should be noted that, across all conditions, election was the preferred procedure, with mean ratings of 5.3 and 3.5 for the election and appointment questions, respectively. Indeed, there were only two discussion groups which did not rate election higher than appointment. Inspection of the means showed that preference for election was consistently high under all conditions, except when "appointment" was seen to produce a highly competent spokesman (Interaction $F = 6.25$, $df = 1/26$, $p < .05$). There were no significant differences as a function of the spokesman's success or failure.

## Discussion

Endorsement of the group spokesman was hypothesized to vary significantly as a function of his source of authority, perceived competence, and task success. These hypotheses received substantial confirmation, although there is clearly a need for clarification of the interactive effects of these variables. The spokesmen seen as more competent and successful were endorsed more strongly than those seen as less competent and unsuccessful, respectively. The hypothesized interactions of perceived competence with source of authority, and success with source of authority, did not occur; there was no difference between the endorsement of the incompetent or unsuccessful spokesman as a function of his election or appointment.

The major question thus remains of how election and appointment qualify endorsement of the spokesman. As noted in the results, there was a tendency to perceive the elected spokesman more positively immediately following the group discussion, although this "halo" did not carry over to the final assessments of member endorsement. In addition, the complex interaction (Figure 1) which emerged among perceived competence, source of authority, and the spokesman's success appeared to indicate that there were important differences in member expectations as a function of election or appointment. For the appointed spokesman, endorsement depended merely on some indication that the "system worked," that is, that it resulted in *either* a competent choice or a successful outcome. For the elected spokesman, however, endorsement depended upon *both* a competent choice and a successful outcome. This result may be interpreted as supporting the general position that election builds higher demands by group members on the leadership role. When the spokesman was appointed, members were more readily satisfied, and responded positively to any sign of a positive result. When he was elected, members failed to support him if he was seen as incompetent or unsuccessful.

If anything, the shape of this interaction indicates that election, rather than making a leader more secure and immune from task effects, creates a situation in which he is more vulnerable to censure. This vulnerability may result from the greater involvement and responsibility which group members have under election conditions, producing stronger feelings of control over the authority structure of the group. Hence, they show a greater willingness to consider alternatives. In the present case, if their first choice of spokesman did not work out, they could try someone else next time around. Under appointment conditions, however, there was no assurance that members would be involved in any subsequent choice of the spokesman, nor was there any definition of the criteria for choosing him. Therefore, group members under appointment conditions may have been more willing to endorse the incumbents.

A second factor which potentially differentiated the appointment and election conditions is the group members' perceptions of the *spokesman's* responsibility for achieving his position. Under elected conditions members may have assumed that the man who became the spokesman voted for himself, whereas under the appointment conditions the incumbent could play no direct role in achieving his position. Therefore, it is the elected spokesman who bears a relatively greater burden of responsibility for his success. This latter interpretation points up an important difference between the appointment condition as created here and that which presumably occurs under many natural settings, where one may question the role an individual plays in "achieving" a desirable appointment.

In addition to the major focus on member endorsement of the spokesman, interesting results were obtained regarding member satisfaction with the verdict and their evaluations of the selection process. Satisfaction with the task outcome was primarily a function of whether the group had received "success" or "failure" feedback. Undoubtedly, there was a strong implicit demand in the experimental procedure which enhanced this effect. Interactions between the success feedback and the expectations established by the spokesman's characteristics, however, cannot easily be explained by these demands. As shown in Figure 2, there was a nonsignificant tendency for success to be more highly valued when it was unexpected, that is, in the case of the low-competence spokesman. This interpretation can also explain the significantly different reactions to failure observed for the elected spokesman who was seen as having either high or low task competence. It is not clear, however, what factors dictated satisfaction with the outcomes achieved by the appointed spokesman, and hence, the whole interpretation remains open to further study.

Finally, with regard to reactions to the leader-selection process, the present findings indicate that election was endorsed regardless of the apparent competence of the man chosen, but that appointment was endorsed only if it was seen to result in a positive outcome.

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# AN EXAMINATION OF THE POWER-INVERSION EFFECT IN THREE-PERSON MIXED-MOTIVE GAMES [1]

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The present study examined the power-inversion effect as it relates to both the truel, that is, a three-person duel, and the triadic coalition-formation situation. Four conditions were examined: (*a*) a deterministic truel which allowed coalitions, (*b*) a deterministic truel which did not allow coalitions, (*c*) a probabilistic truel which allowed coalitions, and (*d*) a probabilistic truel which did not allow coalitions. The main hypothesis was that the power-inversion effect would be replaced by the "strength is strength" effect in the probabilistic coalition-formation situation. It was also predicted that the power-inversion effect would be observed in the truel situations in which no coalitions were allowed to form and in the deterministic coalition-formation situation. The results supported the main hypothesis. However, the support was weakened by the fact that the power-inversion effect was not observed in the deterministic coalition-formation situation. The power-inversion effect was observed in the propensity to attack the stronger of the other two players in the truel situations and in the coalition-formation situations in which coalitions did not form; however, it was not observed in the relative chance for survival.

One of the prominent phenomena predicted by the theories which have attempted to describe behavior in a three-person mixed-motive situation [2] is that the person who is the "strongest" with respect to initial resources is often functionally the weakest. This phenomenon, referred to as the "strength is weakness" effect, has been predicted by theories which deal with coalition formation (Caplow, 1956, 1959; Gamson, 1961b) as well as theories which deal with the truel, that is, a three-person duel (Shubik, 1954). However, the literature has virtually ignored the fact that in many cases the person who is the "weakest" with respect to initial resources is functionally the strongest, that is, the "weakness is strength" effect. It is sensible to think of these two effects as subphenomena of a more general "power-inversion" effect. The present paper examines the power-inversion effect as it relates to both the truel and the triadic coalition-formation situation.

The truel was first discussed by Shubik (1954) and is based on the concept of a three-person duel. It is well known that in a duel, two individuals attempt to eliminate each other and the winner is that person who survives. The truel is a similar situation which differs only in that three individuals participate. In the pure truel no explicit coalitions may form. This does not exclude the formation of implicit coalitions. In fact, in the truel situation it is often the case that two of the participants may increase their chance of survival by eliminating the third participant first and then attempting to eliminate each other.

Due to the paucity of studies dealing with the truel (Cole & Phillips, 1967; Willis & Long, 1967), there is a lack of data to examine the power-inversion effect in that situation. On the other hand, coalition formation has been studied extensively over the last decade and the power-inversion effect has been reported by almost every study (e.g., Gamson, 1961a; Phillips & Nitz, 1968; Vinacke, 1959, 1962; Vinacke & Arkoff, 1957).

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[1] This article is based on research contained in a thesis submitted to Michigan State University, as partial fulfillment of requirements for an MA degree. The author wishes to express his appreciation to J. L. Phillips for his encouragement and criticism during the preparation of the thesis. The research was supported by the Human Learning Research Institute, Michigan State University. A paper based on a portion of the data from this study was presented at the Psi Chi section of the Midwestern Psychological Association convention, May 4, 1968.

[2] The mixed-motive situation is defined as a situation in which (*a*) there is no outcome which will maximize the payoff to all of the participants, and (*b*) at least two of the participants can increase their payoff by cooperative activity.

A coalition has been defined as "the joint use of resources to determine the outcome of a decision in a mixed-motive situation involving more than two units [Gamson, 1964, p. 85]." Coalition theory and research, which are included within the boundary conditions of this definition, have focused on the deterministic coalition-formation situation—those situations in which some unit or combination of units will win with a probability of one. Only two studies (Chertkoff, 1966; Vinacke, Lichtman, & Cherulnik, 1967) have examined the probabilistic coalition-formation situation—those situations in which no unit or combination of units will win with a probability of one.

Chertkoff manipulated the probability of future success by varying the probability that a coalition would win the election after it had won the nomination. In addition he made the payoff contingent upon winning the election. Caplow's (1956) Type 5 triad [$A > B > C, A < (B + C)$] was examined in all conditions. In all three experimental conditions if Player B or C won the nomination, he had a .5 probability of success in the national election. If Player A won the nomination, his probability of winning the national election was .5, .7, or .9, depending upon the condition. A control condition was used in which the probability of future success was not a feature. In the control condition the power-inversion effect was reported. However, in the conditions in which Player A had a .5 or a .7 probability of future success the power-inversion effect was weakened; that is, the distribution of preferred coalition partners was not different from what would be expected by chance. In the condition in which Player A had a .9 probability of future success the power-inversion effect was nonexistent. That is, a "strength is strength" effect was reported. As the probability of future success increased the parity norm became more salient. This was indicated by the tendency for the player with the highest probability of future success to demand and receive a larger share of the payoff.

Vinacke et al. (1967) also examined the differential effects resulting from the probabilistic and deterministic nature of the coalition-formation situation. The major finding of the Vinacke et al., study was that there is a tendency to play without forming a coalition. Moreover, the propensity for a subject to play the game alone increases as the probability that he will win alone increases.

Since the results reported in the probabilistic coalition-formation situation do not follow from any of the existing theories, the present study extends coalition-formation theory to explain individual behavior in the probabilistic situation. The proposed explanation is based on the assumption that in all situations the members of a triad will form that coalition which maximizes their chance of winning. Therefore, in the probabilistic situation the parity norm has little effect in determining which coalition will form. Rather, the uncertainty of the situation fosters a need for security which will be designated the "security principle."

Since any coalition will win with the probability of one in the deterministic situation, the members of a coalition will be secure (i.e., will win) regardless of which coalition is formed. In the deterministic situation the only manifestation of the security principle is that the members of a triad will choose to form a coalition. The preferred coalition will be a function of the parity norm. On the other hand, the probability of winning determines the strength of the players in a probabilistic situation and no player or coalition is guaranteed to win. If all possible coalitions have different probabilities of winning, each player will prefer that coalition which maximizes his chance of winning. To maximize his chance of winning each player must form a coalition with the stronger of the other two players. The strong member of a triad will be included in all preferred coalitions while the weak member will be the least preferred coalition partner. Thus, in the probabilistic situation the "strength is strength" effect is predicted, as opposed to the power-inversion effect which is predicted in the deterministic situation.

Even though the security principle will be salient in the probabilistic situation with respect to the preferred coalition, the parity norm will remain salient for the division of the payoff. This prediction is based on the reasoning that the greater preference for the strong member of the triad as a coalition partner will give him a better bargaining position. Therefore, he will demand and receive the largest share of the payoff.

The present study examined Caplow's (1956) Type 5 triadic situation [$A > B > C$, $A < (B + C)$]. Three players designated A, B, and C participated in a truel game in which the strengths assigned to the players were 4, 3, and 2, respectively. The following four hypotheses were tested:

1. In the probabilistic coalition-formation situation each player prefers the stronger of the other two players as a coalition partner, resulting in AB coalitions being the most frequent.
2. In the deterministic coalition-formation situation each player prefers the weaker of the other two players as a coalition partner, resulting in BC coalitions being the most frequent.
3. In the pure truel situations and those games in which coalitions are permitted but not formed, the power-inversion effect will appear in the following manner. First, there will be a propensity for each player to attack the stronger of the other two players, and second, Player A will win significantly fewer games than Player B, and Player B will win significantly fewer games than Player C.
4. The parity norm will be evident in both the probabilistic and deterministic situations which allow coalitions. That is, the division of the payoff will tend to approximate the ratio of the resources contributed to the coalition by its members.

## METHOD

*Subjects.* The subjects were 255 male undergraduate volunteers who received course credit for their participation. In addition, they were informed that they would have a chance to win $10 if they participated in the experiment.

*Apparatus.* The apparatus for the game was 45 white poker chips; three wooden tokens, one inscribed with an "A," one inscribed with a "B," and one inscribed with a "C"; and a die. Plain white 3 × 5-inch scratch pads were used for all communication, and clipboards were used to allow the communication to remain secret.

*Game design.* The structure of the basic game was as follows. Each of the players began the game with 15 chips. The position that each subject played was determined by chance, that is, each subject drew a token (marked either A, B, or C) from a hopper and played the game in the position indicated on the token. The rules of the game required that each player take away a given number of chips from one of the other two players on each move. Player A took away 4 chips, Player B took away 3 chips, and Player C took away 2 chips. All of the chips that were removed were taken out of the game and did not belong to any of the players. Each player wrote down the letter of the player he wished to attack and gave the message to the experimenter on each move. At no time was a player's choice known before every player had made his choice, that is, the moves in the game were simultaneous. The players were told who had attacked whom after the experimenter had received the choices from all of the players.

*Procedure.* Four conditions were examined: a deterministic game which allowed coalitions (Dc), a pure truel deterministic game (D), a probabilistic game which allowed coalitions (Pc), and a pure truel probabilistic game (P). The same basic truel game was used to keep the conditions as similar as possible while varying enough to allow the game to be defined as either deterministic or probabilistic.

Eighty-five triads participated in the experiment. Five triads (one Dc, one D, two Pc, and one P) were discarded either because the subjects had previous knowledge about the experiment or because two of the subjects in a triad knew each other. Therefore, there were 20 triads in each condition. Each triad played one game. Communication was limited; that is, all messages were written and directed toward the experimenter. A pregame questionnaire was administered to provide a measure of the subject's acceptance of the implied power structure.

In the Dc and D conditions each player took chips away on every move. In the Pc and P conditions whether a player took chips away or not was determined by chance; that is, each player rolled a die after he had decided which player to attack: if the die came up even he took the chips away; if the die came up odd he did not take the chips away. This meant that each player had a 50% chance of taking chips away on each move in the Pc and P conditions.

In the Dc and Pc conditions each subject was given one chance to form a coalition prior to the actual play of the game. They were asked to indicate on a form provided whether they wished to form a partnership or not, and if so with whom. They were also asked to indicate the amount of the payoff they desired and the amount they expected to obtain. If two of the subjects indicated a reciprocal preference, they were given 2 minutes in another room to reach an agreement on how to divide the payoff. The payoff was a $10 prize which the winner or winning coalition in each game had a chance to win in a raffle. If no agreement could be reached in that 2 minutes, the game was played as a truel.

If an agreement was reached, the partnership started the game with 15 chips and the power to take away the number of chips which was equal to their combined power; that is, an AB partnership could take away 7 chips, an AC partnership could take away 6 chips, and a BC partnership could take away 5 chips. This meant that in the Dc condition any partnership was guaranteed to win. In the Pc condition, although a partnership did have a better chance to win than a lone player, a partnership was not guaranteed to win. For example, if an AB partnership was formed in the Pc condition it had a 50% chance of removing 7 chips while C, the isolate, had a 50% chance of removing 2 chips. The expected number of trials for an AB partnership to eliminate Player C was 5 while the expected number of trials for C to eliminate an AB partnership was 16.

In all four conditions the game was over when only one player or partnership had chips remaining. The winner or winning partnership received a chance to win $10 in a raffle.

## RESULTS

### *Results Relevant to Hypothesis 1*

Hypothesis 1 predicted that in the Pc condition each player would prefer the stronger of the other two players as a coalition partner resulting in AB coalitions being the most frequent.

Table 1 presents a who-to-whom matrix for offers in the Pc condition. An analysis by chi-square supported the hypothesis that the stronger of the other two players would be the preferred coalition partner in the probabilistic situation ($\chi^2 = 5.45$, $p < .05$). Further support for the "strength is strength" hypothesis was observed in the significant difference in number of offers received by each player ($\chi^2 = 11.29$, $p < .01$). Player A received more offers than Player B and Player B received more offers than Player C. Since only three coalitions were formed (two AB coalitions and one BC coalition), a test of the second part of Hypothesis 1, that is, AB coalitions will be the most frequent, was not feasible.

### *Results Relevant to Hypothesis 2*

Hypothesis 2 predicted that in the Dc condition each of the players would prefer the weaker of the other two players as a coalition partner resulting in BC coalitions being the most frequent.

Table 1 presents a who-to-whom matrix for offers in the Dc condition. An analysis by chi-square indicated that there was no significant difference from chance expectancy with respect to who preferred whom as a coalition partner ($\chi^2 = 2.63$, $.20 > p > .10$). Further refutation of the power-inversion hypothesis was observed in the distribution of offers received; that is, the distribution of offers received did not differ from chance expectancy. An examination of type of coalitions formed revealed six AB, two AC, and two BC coalitions. Although a satisfactory statistical evaluation of the situation is not feasible because of the small number of coalitions that were formed, it seems clear that the power-inversion hypothesis was not supported.

The data in Table 1 suggested that there was a tendency for the preferred coalition partner to be the same in the Pc and Dc conditions. Since the Pc and Dc conditions were independent, the coalition partner preference in the Dc condition was compared with the coalition partner preference in the Pc condition by computing an *F* ratio of the chi-squares (Lindquist, 1956, p. 39). The *F* ratio implied that there was no significant difference in preferred coalition partner in the Pc and Dc conditions ($F = 2.07$). Chi-squares to test the hypothesis that each player makes the same number of offers in the Pc and Dc conditions revealed a significant difference between players in the Pc condition ($\chi^2 = 7.23$, $p < .05$), but no significant difference between players in the Dc condition ($\chi^2 = .37$, $.90 > p > .80$). An *F* ratio of chi-squares indicated that there was a significant difference between the distribu-

TABLE 1

WHO-TO-WHOM MATRIX FOR OFFERS TO FORM COALITIONS

| Offerer | Pc recipient | | | | Dc recipient | | | |
|---|---|---|---|---|---|---|---|---|
| | A | B | C | Total | A | B | C | Total |
| A | — | 5 | 0 | 5 | — | 8 | 3 | 11 |
| B | 7 | — | 2 | 9 | 8 | — | 5 | 13 |
| C | 10 | 7 | — | 17 | 8 | 6 | — | 14 |
| Total | 17 | 12 | 2 | 31 | 16 | 14 | 8 | 38 |

Note.—Pc condition: $\chi^2_a = 7.23$, $df = 2$, $p < .05$; $\chi^2_b = 11.29$, $df = 2$, $p < .01$; $\chi^2_c = 5.45$, $df = 1$, $p < .05$; Dc condition: $\chi^2_a = .37$, $df = 2$, *ns*; $\chi^2_b = 2.73$, $df = 2$, *ns*; $\chi^2_c = 2.63$, $df = 1$, *ns*: $\chi^2_d = 2.33$, $df = 1$, *ns*. $\chi^2_a$ tests the hypothesis that each player makes the same number of offers. $\chi^2_b$ tests the hypothesis that each player received the same number of offers. $\chi^2_c$ tests the hypothesis that the stronger and weaker players are preferred equally often as coalition partners. $\chi^2_d$ tests the hypothesis that the total number of offers in the Dc condition equals the total number of offers in the Pc condition.

tion of number of offers made in the Pc condition and the distribution of number of offers made in the Dc condition ($F = 19.54$, $p < .05$). Although there was no significant difference between the total number of offers made in the Dc condition and the total number of offers made in the Pc condition, there was a significant difference between total number of coalitions formed in the Pc condition and total number of coalitions formed in the Dc condition ($\chi^2 = 5.61$, $df = 1$, $p < .02$). These data offer support for the suggestion that there is a propensity to play the game without forming a coalition in the probabilistic situation (Vinacke et al., 1967).

### *Results Relevant to Hypothesis 3*

Hypothesis 3 predicted that in the pure truel situations and in those games in which coalitions were permitted but did not form, the power-inversion effect would appear in the following manner: First, there would be a propensity for each player to attack the stronger of the other two players; and second, Player A would win significantly fewer games than Player B, and Player B would win significantly fewer games than Player C. Table 2 presents the frequency with which attacks were directed toward the stronger and weaker player for Conditions Dc, D, Pc, and P on each trial. The stronger attack choice for Player A is Player B and the stronger attack choice for Players B and C is Player A. The strength of each player on each trial was defined as the number of chips he could remove. The number of chips each player had remaining on each trial was not considered for the present analysis.

An examination of the data in Table 2 revealed a significant power-inversion effect on the initial trial in each condition; that is, the stronger player was attacked more often than the weaker player. A trial-by-trial analysis revealed that there was no significant power-inversion effect on any trial after the initial trial in the Dc condition. In the D condition there was a significant power-inversion effect on Trials 2 and 4. In the Pc condition there was support for the power-inversion hypothesis on Trials 2, 4, 7, and 12. In the P con-

TABLE 2

FREQUENCY WITH WHICH ATTACKS ARE DIRECTED TOWARD THE STRONGER AND WEAKER PLAYERS FOR EACH CONDITION ON EACH TRIAL

| | Trial | | | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Condition | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 |
| Dc | | | | | | | | | | | | | | |
| Stronger | 26 | 20 | 16 | 13 | 4 | | | | | | | | | |
| Weaker | 4 | 10 | 14 | 11 | 2 | | | | | | | | | |
| $\chi^2$ | 16.13*** | 3.33 | .13 | .17 | — | | | | | | | | | |
| D | | | | | | | | | | | | | | |
| Stronger | 41 | 39 | 33 | 31 | 9 | | | | | | | | | |
| Weaker | 19 | 21 | 27 | 17 | 6 | | | | | | | | | |
| $\chi^2$ | 8.07** | 5.40* | .60 | 4.08* | .73 | | | | | | | | | |
| Pc | | | | | | | | | | | | | | |
| Stronger | 39 | 36 | 31 | 35 | 29 | 30 | 26 | 21 | 11 | 10 | 5 | 9 | 1 | 2 |
| Weaker | 12 | 15 | 20 | 16 | 22 | 18 | 13 | 15 | 8 | 3 | 5 | 1 | 2 | 1 |
| $\chi^2$ | 14.29*** | 8.65** | 2.37 | 7.08** | .96 | 3.00 | 4.33* | 1.00 | .58 | 1.87 | — | 6.40* | — | — |
| P | | | | | | | | | | | | | | |
| Stronger | 38 | 37 | 39 | 38 | 31 | 35 | 21 | 12 | 11 | 5 | 7 | | | |
| Weaker | 22 | 23 | 21 | 22 | 26 | 19 | 21 | 12 | 7 | 4 | 2 | | | |
| $\chi^2$ | 4.27* | 3.27 | 5.40* | 4.27* | .44 | 4.74* | — | — | .89 | — | — | | | |
| $\chi^2$a | 6.28 | 1.00 | 1.77 | 1.50 | | | | | | | | | | |
| $\chi^2$b | | | | | .18 | | | | | | | | | |
| $\chi^2$c | | | | | | .06 | 1.81 | .40 | .04 | | | | | |

Note.—Only those triads which did not form coalitions were examined in the Dc and Pc condition. $\chi^2$ a ($df = 3$) tests the hypothesis that the distribution of attacks is the same in all conditions on the first four trials. $\chi^2$b ($df = 2$) tests the hypothesis that the distribution of attacks is the same in the D, Pc, and P condition on Trial 5. $\chi^2$c ($df = 1$) tests the hypothesis that the distribution of attacks is the same in the Pc and P condition on Trials 6–9.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

dition the power-inversion hypothesis was supported on Trials 3, 4, and 6.

To test further the power-inversion hypothesis a sign test was used to examine all trials for each condition. If the strong man was attacked more than the weak man a plus was assigned to the trial. In the Dc and D conditions there were five pluses in five trials. The probability that this would have occurred by chance was less than .05. In the Pc condition there were 12 pluses in 14 trials ($p < .01$), and in the P condition there were 9 pluses in 11 trials ($p < .01$).

To compare the distribution of attacks between the four conditions, a chi-square was computed on all trials in which the expected attack frequencies were greater than five (see Table 2). This meant that all four conditions were compared on the first four trials, Conditions D, Pc, and P were compared on Trial 5, and Conditions Pc and P were compared on Trials 6–9. With the exception of the initial trial ($\chi^2 = 6.28$, $.10 > p > .05$) the chi-squares revealed no indication that there was any difference between conditions with respect to the distribution of attacks. When the initial attack data for the combined Dc and Pc conditions were compared with the initial attack for the combined P and D conditions, a significant difference was observed ($\chi^2 = 4.95$, $df = 1$, $p < .05$).

The second part of Hypothesis 3, that is, that Player A will win fewer games than Player B and that Player B will win fewer games than Player C, was partially supported by the data. Table 3 presents the number of games won by each player in each condition. An exact probability test revealed support for the "weakness is strength" hypothesis in the Dc condition ($p = .025$); however, the power-inversion effect was not reported in any condition. An examination of Table 3 indicated that there might be a difference between coalition and no-coalition situations in the distribution of games won. Therefore, the data from the Pc and Dc conditions were combined and the data from the P and D conditions were combined. A chi-square on the combined data indicated that there was a nonsignificant tendency for the power-inversion effect to appear in the truel situations which allow coalitions ($\chi^2 = 5.87$, $.05 < p < .10$). There was apparently no power-inversion effect in the pure truel situations if number of games won was the only datum considered. An $F$ ratio was computed to test the hypothesis that the distribution of games won in the combined Dc and Pc conditions is the same as the distribution of games won in the P and D conditions. The hypothesis was rejected ($F = 34.53$, $p < .05$).

TABLE 3

TRUEL GAMES WON BY PLAYERS A, B, AND C

| Condition | Position |  |  | $\chi^2$ |
|---|---|---|---|---|
|  | A | B | C |  |
| Dc | 2 | 0 | 7 | $p = .025$[a] |
| D | 5 | 6 | 6 | .12 |
| Pc | 2 | 6 | 6 | 2.28 |
| P | 7 | 5 | 7 | .42 |
| Dc & Pc | 4 | 6 | 13 | 5.87 |
| D & P | 12 | 11 | 13 | .17 |

[a] Due to the small expected values in the Dc condition an exact test was computed on those data.

### *Results Relevant to Hypothesis 4*

Hypothesis 4 states that the "parity norm" would be evident in both the probabilistic and deterministic situations which allowed coalitions. That is, the division of the payoff would tend to approximate the ratio of the resources contributed to the coalition by its members.

An analysis of the desired share of the payoff (desired payoff), what each player expected the other player to desire (expected payoff), and the final agreement as to the division of the payoff (final payoff) rejected the hypothesis that the parity norm would be salient in both the Dc and Pc conditions. Table 4 presents the frequency with which the parity split and equal split were reported as the desired payoff and the expected payoff as well as the frequency with which the final payoff approximated the parity split or the equal split. Fifty-fifty splits were classified as equal splits, and parity splits were classified as those splits which were not 50/50 splits and in which the member of the coalition who contributed the most resources received the larger share of the payoff. There were no cases of non-50/50 splits in which the member of the coalition who contributed the most resources received the smaller share of the pay-

TABLE 4

FREQUENCY WITH WHICH THE DESIRED PAYOFF, THE EXPECTED PAYOFF, AND THE FINAL PAYOFF APPROXIMATED THE PARITY SPLIT OR WAS AN EQUAL SPLIT IN THE PC AND DC CONDITIONS

| Payoff | Condition | | | | | |
|---|---|---|---|---|---|---|
| | Dc | | | Pc | | |
| | Parity split | Equal split | $\chi^2$ | Parity split | Equal split | $\chi^2$ |
| Desired | 6 | 32 | 17.79* | 8 | 23 | 7.25* |
| Expected | 5 | 33 | 20.63* | 7 | 24 | 9.19* |
| Final | 2 | 8 | 3.60 | 2 | 1 | — |

* $p < .01$.

off. Chi-squares computed on these data revealed that with the exception of the final payoff there was a significant tendency ($p <$ .01) for an equal split in both the Dc and Pc conditions. Moreover, there was a nonsignificant tendency toward an equal split of the final payoff in the Dc condition ($\chi^2 =$ 3.60, $.10 > p > .05$). There was an insufficient amount of data to compute a chi-square for Pc final payoff.

## DISCUSSION

The main hypothesis of this study was that the power-inversion effect, which has been reported in previous coalition research, would be replaced by a "strength is strength" effect in the probabilistic coalition-formation situation. The results with respect to preferred coalition partner in the Pc condition supported the main hypothesis (Table 1). However, contrary to what was expected, the power-inversion effect was not observed in the control condition, that is, the Dc condition. In fact, although it was not significant, the distribution of preferred partner choices in the Dc condition reflected a "strength is strength" effect. Furthermore, there was no significant difference between preferences for coalition partner in the Dc and Pc conditions. Considering the data, it is apparent that a slight "strength is strength" effect was obtained in the Dc condition.

That the slight "strength is strength" effect in the Dc condition weakens the support for the hypothesis that the "strength is strength" effect is a function of the probabilistic situation cannot be denied. Because of the results in the Dc condition, an adequate test of the hypothesis that the "strength is strength" effect is a function of the probabilistic situation has yet to be conducted. Moreover, the hypothesis that the power-inversion effect is a function of the deterministic situation will require further research. The fact remains that the probabilistic condition of the present study, Chertkoff's (1966) study, and the Vinacke et al. (1967) study are the only studies which have utilized a probabilistic game in a triadic coalition-formation situation. Chertkoff's study and the present study obtained a statistically significant "strength is strength" effect, and Vinacke et al. did not report data which allowed the "strength is strength" hypothesis to be examined. All of the other studies dealing with coalition formation have been deterministic, and virtually all of them have reported the power-inversion effect. In addition, none of them have obtained a significant "strength is strength" effect. Therefore, it is argued that research to date indicates that the "strength is strength" effect and the power-inversion effect are a function of the probabilistic-deterministic nature of the situation. It is apparent, however, that further research is required before support for such a statement can be considered conclusive.

Unlike previous studies which have reported that coalitions were formed virtually every time they were allowed, the present study obtained only 13 out of a possible 40 coalitions. There are indications that the small number of coalitions formed was partially an artifact of (*a*) the requirement that reciprocal contacts had to be made before coalitions could be formed, and (*b*) the requirement that only one attempt to form a coalition was permitted per game. However, the differential number of coalitions in the Pc and Dc conditions indicates that there is another factor that has some effect on number of coalitions formed. The results of the Vinacke et al. (1967) study suggest that such a factor is the tendency for the subjects to "gamble," to risk winning on their own. The small number of coalitions formed in the present study may also have been a function of the tendency to gamble. The reason that significantly fewer coalitions were formed in the Pc condition than in the Dc condition apparently rests on the differential propensity for Player A to gamble in the probabilistic and deterministic games. A nonsignificant tendency for Player A to gamble more in the Pc condition than in the Dc condition was noted. This probably resulted from the fact that in the Pc condition Player A had a chance to win if a coalition formed against him while in the Dc condition a BC coalition would win with a probability of one. Therefore, in the Dc condition Player A had to take into account the possibility that the other two players could form a coalition and leave him the loser.

The results of the present study indicated that there was no difference between the situations in which a coalition did not form when allowed and the pure truel situations with respect to strategies employed; that is, both types of situations (Table 2) reported a significant propensity to attack the stronger attack choice. However, when the distribution of games won was examined, there was a nonsignificant tendency toward the power-inversion effect in the combined Pc and Dc conditions which was not observed in the combined P and D conditions (Table 3). There was also a significant difference between the distribution of games won in the conditions which allowed coalitions and the conditions that did not. The fact that the games were seemingly played the same and that the outcomes were different posed an interesting question. One possible resolution of this question is given below.

In the Pc and Dc conditions the subjects were given a chance to form a coalition. Those situations pertaining to the effect in question were the ones in which the subjects chose to play the game as a truel. However, because the subjects were given the chance to form a coalition, they realized that two players could form an implicit coalition and overpower the third. They also realized that Player A had the greatest power to remove chips. Therefore, on the first move Players B and C formed an implicit coalition which was reflected by a significantly greater propensity to attack the stronger attack choice on the initial move in the Pc and Dc conditions than was observed in the pure truel situation. Some support for the above reasoning was found in the attack data given in Table 2 of the Results section. A chi-square comparing the combined Pc and Dc initial trial attack data with the combined P and D initial trial attack data revealed that the propensity to attack the stronger attack choice was significantly greater in the conditions which allowed coalitions than in the pure truel conditions.

After the initial trial there was no difference between the conditions which allowed coalitions and those that did not allow coalitions with respect to the propensity to attack the stronger attack choice. This probably resulted from the confounding of the power of each individual after the initial trial. On every trial after the initial trial the power of each individual is a combination of the number of chips he has remaining as well as his ability to remove chips. The greater initial propensity to attack the stronger attack choice in the Pc and Dc conditions resulted in a distribution of chips which favored the weakest player. Therefore a nonsignificant power-inversion effect was observed in the Dc and Pc conditions.

In the P and D conditions each player attempted to maximize his chance of winning. Since they had not been offered the opportunity to form an explicit coalition, they did not immediately consider the possibility of forming an implicit coalition. Instead, they devised strategies based on their individual power. The major strategy was for each member of the truel to attack the stronger attack choice and eliminate him before he eliminated them. This is reflected by the significant tendency to attack the stronger attack choice over all trials. However, another strategy on the initial trial would be to attack the weaker attack choice in hopes of drawing fewer reprisals from the stronger attack choice. It is proposed that in the P and D conditions some individuals employed the second strategy on the initial move. As a result, the distribution of chips on the second trial did not favor the weakest member of the truel to the extent that it did in the Pc and Dc conditions. Therefore, the distribution of games won in the P and D conditions did not differ from chance expectancy.

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# FORMATION OF CONTRACTUAL AGREEMENTS BETWEEN PARTIES OF UNEQUAL POWER[1]

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This study attempted to identify the effects of legitimization of role assignment, size of bargaining party, and attractiveness of independent, alternative outcomes on the formation of restrictive agreements in mixed-motive bargaining. Although the incidence of agreement formation did not differ as a function of the experimental manipulations, it was possible to identify differences and similarities between these findings and previous ones. Groups with an attractive alternative to interdependent negotiation formed agreements regulating their bargaining because of their awareness that agreements restricted the power of each to disrupt their joint attainment of maximum outcomes. Groups with an unattractive alternative were able to form restrictive agreements only when the low-power party discovered a tactic for compensating its high-power opponent for relinquishing its power.

A number of recent studies (Murdoch, 1967; Thibaut, 1968; Thibaut & Faucheux, 1965) have shown that formal (contractual) agreements are formed by the parties to a mixed-motive relationship as a way of controlling disruptive disturbances that endanger the attainment of satisfactory joint outcomes. Although these studies differ considerably in procedure and in subject characteristics, their results are in good agreement in finding the highest incidence of such contractual agreements to occur when there exist a high conflict of interest and, simultaneously, a highly attractive alternative to the bargaining relationship. These conditions jointly constitute a situation in which it is plain to both sides that one party is strongly tempted to be exploitative and the other to be disloyal. An enforceable agreement prohibiting both kinds of disruption protects each from the other and thus permits joint outcomes to be maximal.

To make the analysis more explicit, suppose that two parties confront one another in such a bargaining situation where only through interdependence can they gain maximum outcomes. Party A has an exploitative capability which he is strongly tempted to use, and Party B is strongly tempted to defect. Insofar as each party can perceive the basis for the other's temptation, each has a credible threat the relinquishment of which he may be willing to exchange for the other's like action. Formal agreement is not difficult to reach.

But when Party B's alternative to the relationship is plainly unattractive, the credibility of any threat to withdraw from the negotiations is sharply reduced. The present experiment focuses on the predicament of Party B in this vulnerable, threat-deprived position—on the strategies and tactics available to him in striving to protect himself from A's exploitation. Are there any conditions in which a resourceful B can offset his lack of credible threat, by somehow compensating A for assent to a formal agreement that puts restraints on the use of their (vastly unequal) capacities to inflict damage? What are the minimal terms that each party would have to insist on in order to find a restrictive agreement acceptable? These are the questions addressed by the present study.

To provide a setting within which one might hope for an answer to these questions, our experiment holds constant at a high level the conflict of interest between the parties, and varies three factors relevant to the relative power of the two parties: (*a*) The power of Party B is directly manipulated by varying, from low to high, the *attractiveness of the alternative* to the bargaining relationship; (*b*) the *legitimacy* of the assignment of the parties to their power positions varies in two degrees; this might be thought of as manipulating the degree of acceptance of differentials in power; (*c*) the strength of each bargainer is manipulated by providing him (or not providing him) with a partner or *constituent* with whom he acts in coalition.

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## Method

### *Subjects*

The subjects were male volunteers from introductory psychology and political science courses at the University of North Carolina. Half of the groups run were dyads and half were double dyads. The 240 subjects yielded 10 groups in each of the eight cells of a 2 × 2 × 2 design. One experimenter[3] ran 4 groups and the other (CLG) 6 groups in each of the cells. The attempt was made to assign subjects to cells at an equal rate, but when fewer than four kept their appointment a dyad was run in place of a double dyad.

### *Procedure*

In addition to providing a brief description of the "scenery" of the bargaining game, the instructions implemented the experimental manipulations. Subjects were told that they would represent two nations by negotiating annual trade agreements between them. They were also told the rate at which each subject would earn money: $.01 for each $1 million received by his nation. The two nations were Upper Fenwick (Fen), a "large, relatively industrialized nation with plenty of power available to it," and East Arkan (Ark), a "small, underdeveloped, mountainous country which has relied primarily on tourists to support its national economy."

*Random-legitimate manipulation.* The experimenter gave the subjects 10 minutes to complete an "International Economic Policy Test." The 15 multiple-choice items on this test had been chosen on the basis of their strong face validity for questions regarding the conduct of a nation's international economic policy. The experimenter's explanation of the test after subjects had completed it constituted the ostensible basis of their assignment to represent the two nations.

Half of the subjects (random treatment) were told that their tests would be scored after the negotiations, and would then be compared with their performances in bargaining to determine the relationship between the two. These subjects were publicly assigned to the two nations on a random basis.

The other half of the subjects (legitimate treatment) thought that they were assigned to represent the two nations on the basis of their test scores. One (dyad treatment) or two (double dyad treatment) randomly selected subjects were told that they had scored higher and that this entitled them to represent the nation with the superior bargaining position (Fen). As a result, they could possibly earn more money than the subjects representing Ark. The intention of this manipulation, of course, was that subjects in the legitimate treatment should perceive the assignment as being based on individual merit rather than on chance.

*Dyad–double-dyad manipulation.* The two parties were located in adjacent rooms with a large window in the common wall. By means of an intercom, the representatives were in verbal contact with each other and with the experimenter, who monitored the conversation from a third room.

In the double-dyad treatment, each pair of representatives was divided into *spokesman* and *constituent* by the flip of a coin. The spokesmen did all the talking for their nations; they were allowed to talk to each other, and each was allowed to speak privately to his own constituent. Constituents were seated to the side of the common window, and each was allowed to address only his own spokesman, although he heard the conversation between the spokesmen.

*High-alternative–low-alternative manipulation.* Each nation had to decide whether or not to trade with the other for each year; the underdeveloped country had the raw material, while the other possessed the technical and financial wealth to process the ore into a valuable "super alloy." There were $98 million in profits available each year, to be divided between the two nations if they both decided to trade with one another. The matrices in Figure 1 described the situation facing the subjects. They did not see these matrices—all choices were explained verbally. If a nation decided not to trade, but rather to take its alternative source of income that year, it received either $10 million (low alt) or $35 million (high alt). If one decided to trade while the other chose its alternative, the one which decided to trade received nothing that year. If both decided to trade, the total profit for that year ($98 million) was divided between the two nations as specified by Fen; this was the nature of its power. The maximum any one nation was allowed each year was $88 million, and the minimum was $10 million.

*Steps in the negotiations.* The two parties first negotiated verbally to a "tentative agreement" regarding the division of the $98 million; neither nation was bound to adhere to this agreement. Subsequently, each party privately made an "individual decision" either to trade or to receive its alternative; it was at this point that Fen could exercise its power to change the division of the joint outcome from that specified in the tentative agreement. The final outcome of the negotiation was determined by the interaction of the individual decisions, as described in the preceding section.

Subjects were allowed a practice negotiation, followed by any necessary clarification of the rules

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[3] The authors are indebted to Douglas R. Raichle for his assistance in carrying out this study.

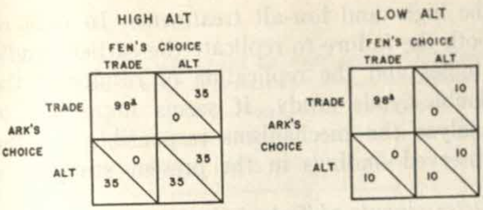

FIG. 1. Matrices depicting the choices facing subjects and their outcomes for each fiscal year. (a. If Fen chose to trade, it had the task of specifying how the $98 million should be divided between the two nations; the $98 million was divided as Fen specified if Ark also chose to trade. Divisions had to be in multiples of $1 million, and either nation could receive a maximum of $88 million or a minimum of $10 million.)

and procedure. They then carried out the first 3 "years" of negotiation.

*Contracts.* There were two changes in procedure made for the second 3 years. Subjects negotiated only once to determine the outcome for all 3 years. Their decisions were worth three times as much as each previous one had been.

Subjects were also given the opportunity to form a contract regulating their bargaining for this session. A contract cost each nation $6 million (and, hence, each subject $.06) for the 3-year period. The two parties had to negotiate whether to have a contract along with reaching a tentative agreement for the period.

Before commencing these negotiations, subjects completed a "Personal Preference Inventory" in order to become familiar with the three types of contracts it was possible to form. They were asked to indicate (on forms listing all possible outcome allocations) the smallest share of the total outcome they would be willing to accept with each of the types of contracts.

A Type 1 contract was designed to be most appealing to Ark; it prohibited Fen from changing the division of the $98 million from the tentatively agreed-upon division. A Type 2 contract prohibited either nation from choosing its alternative source of outcomes during the negotiations or in its individual decision. It was most restrictive of Ark's bargaining position by eliminating this nation's only offensive and defensive maneuver. A Type 3 contract combined both rules explicit in Type 1 and Type 2 contracts; it prohibited Fen from changing the tentatively agreed-upon profit division and either nation from choosing its alternative. This last was thought to be the contract most likely to be formed. Under its terms, each nation made the public sacrifice of relinquishing its right to perform potentially disruptive behavior. Each might offer to form such a contract, it was felt, in order to induce the other to do the same and in this way to gain control over the other's behavior and to assure its own outcomes.

After completing the Personal Preference Inventory, the two parties decided which contract to form, if any, reached a tentative agreement, and the negotiations were concluded. These contract negotiations were tape-recorded.

Subjects then completed a postexperimental questionnaire and were paid the money they had earned. (Each representative of each nation earned money at the rate of $.01 for every $1 million received by his nation; hence the maximum earnings any subject could conceivably receive was $5.28.) The experimenter interviewed them to discover if any harbored suspicions of the study's true nature. He then gave a brief explanation of its purpose, pledged them not to discuss it with others, and dismissed them.

## RESULTS

### *Checks on the Experimental Manipulations*

*Frequency of withdrawing to the alternative sources of outcomes during the first 3 years.* Subjects in the high-alt treatment had a more attractive alternative (35) than those in the low-alt treatment (10). The high-alt subjects chose the alternative significantly more often during the first 3 years (mean number of choices of alternative out of a possible three was .550) than did the low-alt subjects (.325); $F = 5.01$, $df = 1/144$, $p < .03$.

*Random versus legitimate assignment to the Fen and Ark positions.* On the postexperimental questionnaire, subjects indicated how they perceived their assignment to these roles to have been determined: On a 41-point scale the extreme left end (rating of 0) was labeled "completely arbitrary" and the extreme right end (rating of 40) was labeled "based entirely on merit." Subjects in the legitimate treatment rated their assignment as having been determined more by "merit" than did subjects in the random treatment. The mean rating by legitimate subjects was 22.24, while the mean rating by random subjects was 2.46; the difference between these means is significant at the .001 level, $F = 313.49$, $df = 1/126$.

*Power differential between Fen and Ark.* The mean per-year outcomes to Fen and Ark for Years 1–3 provide a check on Fen's superior control of the outcome division. Each point earned represented $1 million in the scenery of the negotiations and $.01 in payoff to each subject. Across all conditions the mean per-year outcomes for Fen and Ark were 49.94 and 33.99, respectively. The difference between these mean outcomes is significant with $F = 71.02$, $df = 1/144$, $p < .001$.

*Assignment to spokesman and constituent* *roles in double dyads.* Some idea of how these roles were experienced by the subjects holding them can be gained from the indication on the postexperimental questionnaire of having "any desire to change roles." Only 21 of the 81 spokesmen responded "yes," while 59 of the 80 constituents did so. It is quite clear that spokesmen did not want to have the less active role, while constituents wished that they had been given the more important position for their nation. The observed pattern of frequencies yields a chi-square of 34.22, $p < .001$.

*Frequencies of Type 3 Contracts*

Frequencies of Type 1 and Type 2 contracts were so small that we focus exclusively on Type 3 contracts. Type 3 contracts were those containing provisions that prohibited both "exploitation" and withdrawal to the alternative. "Exploitation" involved any self-aggrandizing change by Fen of the agreed-upon profit division; this prohibition clearly protected Ark. Prohibiting withdrawal to the alternative protected both parties, but since withdrawal was Ark's sole threat, its prohibition was particularly reassuring to Fen. Frequencies of Type 3 contracts in various conditions are shown in Table 1.

There were no large or consistent differences due to the experimental manipulations in the number of groups forming a Type 3 contract. In earlier experiments with dyads (Murdoch, 1967; Thibaut, 1968; Thibaut & Faucheux, 1965), which were most similar to the random treatment here, there were more Type 3 contracts formed in high alt than in low alt. In an experiment (Thibaut, 1968) with double dyads, also similar to the present random treatment, there were no differences in the number of Type 3 contracts formed in the high- and low-alt treatments. In light of both the failure to replicate the earlier dyadic studies and the replication of results of the double-dyads study, it seems important to analyze the mechanisms responsible for the observed findings in the present study.

TABLE 1

FREQUENCIES OF TYPE 3 CONTRACTS

| Alternative level | Dyads | | Double dyads | | Totals |
|---|---|---|---|---|---|
| | Legitimate | Random | Legitimate | Random | |
| High alt | 8 | 5 | 6 | 7 | 26 |
| Low alt | 7 | 5 | 5 | 8 | 25 |
| Totals | 15 | 10 | 11 | 15 | 51 |

*Determinants of Type 3 Contracts*

*Frequency of choosing the alternative during precontract years.* Those groups that subsequently formed a Type 3 contract made 1.16 alternative choices as opposed to only .38 such disruptions in non-contract-forming groups. This difference is significant by analysis of variance, $F = 10.29$, $df = 1/128$, $p < .002$. Alternative choosing was more frequent in high alt than in low alt (see Checks on the Experimental Manipulations). Therefore, it would seem that, as in the earlier studies, the occurrence of this type of disruptive behavior during the precontract period may account for the contracts formed in high alt. However, this provides no clue for understanding the contracts formed in low alt. In the earlier studies with dyads fewer contracts were formed in low alt than in high alt, while with double dyads the number of contracts in the two treatments did not differ.

*Frequency of exploitations of Ark by Fen during Years 1–3.* This frequency was greater in groups that subsequently formed Type 3 contracts (mean number of exploitations = .63) than in groups not doing so ($M = .41$). Although this difference was marginally significant ($F = 3.72$, $df = 1/64$, $p < .06$), there were no significant differences between the frequencies of this kind of disruptive behavior in high and low alt treatments. Hence this finding is not helpful in attempting to account for contracts in low alt.

*Outcomes to Fen and Ark.* In further pursuit of an understanding of the processes underlying contract formation in low alt, it is necessary first to consider some of the data relevant to the bargaining outcomes to Fen and Ark during Years 1–3. Table 2 presents these data for the parties which formed a Type 3 contract in the high-alt and low-alt treatments.

First, note from Columns 1 and 2 that, for these contracts-forming groups, as for all groups, Fen gained a larger share than did

TABLE 2

DATA RELEVANT TO BARGAINING OUTCOMES FOR GROUPS WHICH FORMED A TYPE 3 CONTRACT

| Alternative level | *M* outcomes per-yr. for Yrs. 1–3 | | Least Fen will accept (from Personal Preference Inventory) | Most Ark will give Fen (from Personal Preference Inventory) | 1st offer by Ark to Fen (from tapes) | Amount Allocated to Fen in contract |
|---|---|---|---|---|---|---|
| | Fen | Ark | | | | |
| | I | II | III | IV | V | VI |
| High alt | 45.59$^a$ | 33.53$^a$ | 52.92$^a$ | 51.96$^b$ | 49.76$^a$ | 52.25$^a$ |
| Low alt | 49.81$^a$ | 26.86$^b$ | 57.63$^a$ | 59.71$^b$ | 55.08$^b$ | 56.71$^a$ |

$^a$ $N = 25$.
$^b$ $N = 24$.

Ark of the outcomes attained during the initial bargaining period. Futhermore (also as in the entire sample of groups), Fen's share was disproportionately large under low-alt conditions. The interaction in Columns 1 and 2 is significantly by analysis of variance ($F = 4.27$, $df = 1/83$, $p < .04$).

Note next from Column 3 that the minimum share of outcomes that would make a Type 3 contract acceptable to Fen was greater in low alt than in high alt; this difference is significant ($F = 6.29$, $df = 1/40$, $p < .02$). From Column 4 it is clear that Ark was willing to concede to Fen a larger share of the outcomes in low alt than in high alt ($F = 21.24$, $df = 1/40$, $p < .001$).

That Ark in low alt did, in fact, begin the bargaining for Years 4–6 with some intention of offering Fen a disproportionately large share of the total is corroborated by the data in Column 5. The mean size of the first offer that Ark made to Fen at this time was obtained from the tape-recorded[4] negotiation sessions. Ark offered significantly more in low alt than in high alt ($F = 13.82$, $df = 1/69$, $p < .001$).

The outcome of the negotiations for Years 4–6 for groups forming Type 3 contracts is presented in Column 6. Low-alt Fens received significantly higher outcomes than did high-alt Fens, as expected, given the attitudes and behavior of Fen and Ark during the negotiations preceding this one (Columns 1–5). The $F$ ratio ($df = 1/40$) was 10.08, $p < .003$.

Ark has apparently learned during Years 1–3 that he has a limited counterpower over Fen, and that he has to give Fen a relatively larger share of the outcomes than he himself receives. To prevent the possibility of rampant exploitation by Fen, it is in Ark's interest to form a Type 3 contract. In high alt it is apparently possible for Ark to convince Fen that such a contract would be mutually profitable since (in addition to preventing Fen from exploiting Ark) it would prevent Ark from withdrawing to his alternative. In low alt, however, Fen and Ark both realize that Ark's alternative is not more profitable than trade with Fen (and, hence, is not a rational choice). Since contracts are more advantageous to Ark than to Fen in low alt, it is reasonable to surmise that Ark initiated the proposal to form a contract.

If Ark strives to form a contract with Fen in the low-alt condition, what must he do? Generally, Ark must shift the focus of bargaining from a preoccupation with mutual *threat* (he has nothing very credible in this line) to a focus on the positive advantages to be gained by Fen through forming a contract. Only by doing so can the low-alt Ark persuade Fen that the gains from a contract outweigh the losses from restricted power. To accomplish this tactic, Ark must do two things during the face-to-face negotiations over contracts. He must (*a*) link or "couple" (Kelley, 1966; Fisher, 1964) specific outcome allocations with the adoption of a Type 3 contract, and (*b*) offer an outcome allocation that meets or exceeds Fen's minimum requirement.

It is suggested, then, that groups forming Type 3 contracts will more frequently in low alt than in high alt contain Arks who show both elements of the hypothesized tactic: (*a*) During the tape-recorded negotiations, they will couple outcome allocations with contracts of Type 3 either by associating the two in a "packaged" proposal to Fen, or by immediately countering Fen's suggestion of an outcome allocation with the suggestion of a Type 3 contract, or by immediately countering Fen's suggestion of a Type 3 contract with the suggestion of an outcome allocation; *and* (*b*) during the tape-recorded negotiations, they will offer outcomes to Fen that equal or exceed the amount Fen has recorded on the Personal Preference Inventory as the least he would require in order to find a Type 3 contract acceptable. Both elements of the tactic were taken to be fulfilled, then, if Ark completed the specified temporal pairing of an outcome allocation with a Type 3 contract and also proposed a division of the profits that exceeded Fen's known "break-off" value.

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[4] The authors are indebted to Cynthia Felgar for transcribing and coding these data (and the others subsequently referred to in this report) from the tape recordings. She was unaware of the hypotheses of the present study and of the results of the earlier ones.

To test this proposal, let us first examine in the low-alt condition the proportion of groups in which Ark employed both elements of the tactic among groups forming Type 3 contracts, and among groups not forming such contracts. While Arks performed the full tactic in 16 of the 24 contract-forming groups, in only 1 of the 15 groups failing to form contracts did they do so. This pattern of frequencies yields a chi-square of 11.18, $p < .01$. In the high-alt condition, the results of this comparison do not reach statistical significance.

Hence, it is only in the low-alt condition that the occurrence of the tactic discriminates between groups forming and not forming contracts. Furthermore, when the analysis is restricted to groups forming Type 3 contracts, the proportion of low-alt Arks who employ both elements of the tactic (67%) is almost twice that of those in high alt who do so (9 out of 25, or 36%). This difference is significant by chi-square (4.61; $p < .05$).

## DISCUSSION

The implications of the results of the present study seem clear in their broad outlines. In a mixed-motive relationship characterized both by sufficient common interest to encourage negotiation and by sufficient bases for exploitation and disloyalty to endanger its viability, formal contractual agreements can be reached by (at least) two widely different processes. The first of these appears to account for contract formation in the present high-alt condition, as well as for the main findings of the earlier experiments on contract formation, cited above. When the parties to the relationship are nearly equal in power, and hence when mutual threat is high and credible, the contractual agreements are reached through mutual recognition by the parties that contracts provide and will enforce an exchange of threat reduction.

The second process may account for the findings in the low-alt condition of the present study, as well as for those of the earlier study of contract formation in double dyads. It is the nature of this process that we wish now to explore. The present data suggest that in the low-alt condition Ark has somehow discovered a tactic which enables him to reach contractual agreements with Fen more surely than if he had restricted himself exclusively to the use of threat. Since he can make his threat credible to Fen only by considerable sacrifice to himself, the discovery of this more efficient method for extracting contracts may seem quite elementary.

Nevertheless, though resorting to threats is clearly ineffectual and self-defeating, the possible alternatives to this posture may not be at all salient. Ark may be captured by Fen's definition of the relationship as one being governed by threat, as exemplified in Fen's aggressively exploitative behavior, particularly since in the precontract period there exists no reliable device for controlling Fen's threat. Even if Ark strives to break out of this threat-polarized tradition, he may find no better solution to the problem.

Although the present experiment demonstrates that Arks deprived of credible threats can improvise a means of obtaining contracts, it can only suggest some of the circumstances that promote this improvisation. In general these favorable circumstances would seem to include any factors that either would reduce the competitiveness of the relationship, thus facilitating the shift away from mutual threat, or would actively stimulate the coupling of outcome allocations with the formation of contracts. The present experiment differed from the earlier ones both in the use of a monetary incentive and in the interpolation of the Personal Preference Inventory between the precontract and the contract periods.

A monetary incentive may under some conditions (e.g., as in Gallo, 1966, and Kelley, 1965) reduce the degree of competitiveness in the relationship and lead to the exploration of "rational" ways of maximizing one's own outcomes. While we cannot dismiss the possibility that the monetary incentive contributed to our results, we find no basis for believing that the interactions in the present experiment were less competitive than in the earlier studies. Where relevant comparisons can be made, exploitative and disloyal behavior occurred no less frequently in the present experiment than in the earlier ones. (However, even given equally competitive behavior it would be possible to argue that in the present experiment the recipient of such behavior may not perceive it as being under the control of a motive to hurt him but rather as being under the external control of the monetary incentive. The attribution to the other of a motivation to maximize his own monetary outcomes may seem quite legitimate, and hence might permit the recipient to make a noncompetitive definition of the situation.)

A more plausible case may perhaps be made for the Personal Preference Inventory as being responsible for the emergence of Ark's tactic in the low-alt condition. The sheer cognitive pairing of all possible outcome allocations with all possible contracts may have been sufficient to suggest a good part of the necessary tactic to these threat-deprived Arks.

We have noted above that only in dyads is there a discrepancy between the results of the present experiment and the earlier ones. The earlier experiment composed exclusively of double dyads and the double dyad condition in the present experiment are in agreement in showing as many contracts being generated in the low-alt as in the high-alt conditions. There is a hint in the present data that the availability of an active collaborator, as in the double dyads, leads to the production of more suggestions about tactics for gaining contracts than the isolated individual can produce. A count of the frequencies of Ark's "coupling" outcome allocations with Type 3 contracts in the tape recordings of contract negotiations for the entire sample of groups reveals that, while half of the dyads performed such coupling, over 70% of the double dyads exhibited this tactic. This difference in incidence is only marginally significant ($\chi^2 = 3.21$, $p < .10$). Nevertheless, it is possible to speculate that the presence of a constituent in the earlier double-dyad experiment may have stimulated tactical, problem-solving behavior in much the same way as, in the present one, we have suggested the Personal Preference Inventory and possibly the financial incentive to have functioned. This possibility is enhanced by the findings of Pylyshyn, Agnew, and Illingworth (1966). These investigators demonstrated that dyadic bargaining parties behaved more rationally and cooperatively in a Prisoners' Dilemma game than did individuals. They also speculated that this may have been due to the superiority of groups over individuals in problem solving.

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# EFFECT OF INCENTIVE MAGNITUDE ON COOPERATION IN THE PRISONER'S DILEMMA GAME[1]

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Recent criticism of research using the Prisoner's Dilemma (PD) game suggests that previously obtained results may be seen as an artifact of the typical use of "trivial" monetary incentives in the game. Critics propose that PD subjects under imaginary or trivial incentive conditions compete primarily because they become bored with cooperation. The present experiment varied incentive magnitude for subjects playing the same basic 20-trial PD game. In five experimental conditions, subjects played for imaginary dollars, small and intermediate amounts of real money, and real dollars. Results indicated that: (*a*) Cooperation tended to decrease over time in all conditions; (*b*) subjects who played for real money played quite competitively regardless of incentive magnitude; and (*c*) subjects who played for real dollars were significantly more, rather than less, competitive than were subjects who played for imaginary dollars. The results were interpreted as failing to support the "insufficient incentive to cooperate" criticisms.

In recent years a two-person mixed-motive game known as the "Prisoner's Dilemma" (PD) has come to be widely used in the investigation of various factors contributing to the cooperative or competitive resolution of human conflict. Aspects of communication, decision making, motivation, person perception, and personality have been studied through use of the PD game; data collected in this format have been used to support a considerable number of propositions about conflict resolution (see a review by Gallo & McClintock, 1965).

The basic PD game matrix used in the experiment we report below is illustrated in Figure 1. The gains or losses incurred by the players on each trial are dependent on choices made by both of them between two available behaviors. Typically, PD research in which pairs of subjects make several successive game choices has shown a dramatic decrement in the amount of mutual cooperation (the $a_1$ $b_1$ choice) over trials; subjects generally have great difficulty maintaining cooperation and thus realizing maximum joint earnings. PD researchers have generally interpreted these results as indicating that players are tempted, at least from time to time, to try to maximize their own short-term gains at the expense of the other by making the $a_2$ or $b_2$ (competitive) choice. Thus, they tend to perceive the other player as similarly tempted, and may begin to choose competitively out of a desire to defend against loss at the hands of the other as well as out of a desire to maximize their own gain. The essential psychological feature of the game, then, is that unless the conditions for mutual trust exist for the players (Deutsch, 1962) their behavior will tend to stabilize in the $a_2b_2$, or mutually competitive, cell of the matrix.

Gallo and others (Gallo, 1966; Gallo & McClintock, 1965; Kelley, 1965; McClintock & McNeel, 1966b) have proposed that since previous research within the framework of the PD game has typically been conducted using very small or "imaginary" incentives for play, the research is subject to two related alternative interpretations. One interpretation suggests that subjects become bored with making long sequences of mutually cooperative choices for rewards of only trivial value, and therefore convert the game to a more challenging one in which the object is to maximize the difference between their own payoffs and those of their opponents.[3] The second interpretation

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[1] The research reported in this paper was partially supported by the Office of Naval Research under Contract Nonr-4294(00) to Morton Deutsch.

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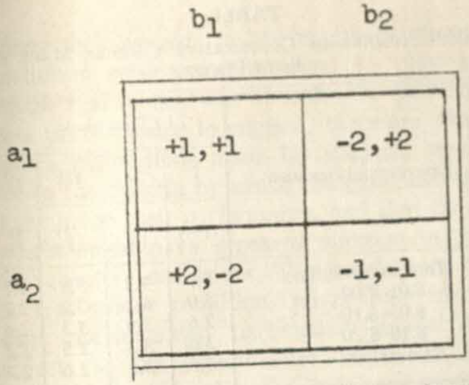
[3] Messick and Thorngate (1967) have presented evidence that even in games in which cooperation is easy to achieve and maintain players often reduce their potential outcomes by taking into account the



|  | b1 | b2 |
|---|---|---|
| a1 | +1,+1 | -2,+2 |
| a2 | +2,-2 | -1,-1 |

FIG. 1. Basic PD matrix used in this experiment.

suggests that at the beginning of the game experimental subjects fail to understand fully the consequences of their choices and thus begin by experimenting with various strategies. "By the time the subject learns the implication of what he is doing, if he ever does, it is probably too late to break out of the competitive pattern that has been established, particularly if motivation to do so is minimal or entirely lacking [Gallo & McClintock, 1965, p. 76]."

We are particularly concerned, in this paper, with the first of these interpretations. It suggests that an implicit assumption of most researchers who have used the PD game —that experimental subjects take the game and the experimenter's instructions seriously —may not be tenable where monetary-incentive magnitude is low or imaginary. Since this interpretation has received some measure of empirical support, it is particularly damaging to much previous PD research. The empirical support comes from a study by Gallo (1966), in which subjects played a game similar in some respects both to the PD game and to the trucking game devised by Deutsch and Krauss (1960, 1962). In Gallo's experiment, subjects who played for imaginary money behaved in typically competitive fashion, while subjects who played for a sizable amount of real money played the game far more cooperatively. Gallo interpreted his results as calling into question the interpretation by Deutsch and Krauss (1960, 1962) of their experiments on the effects of threat availability on the outcome of interpersonal bargaining. Although subsequent research using the trucking game with moderately large monetary incentives has failed to support Gallo's alternative interpretation (e.g., Brown, 1967; Gumpert, 1967), the PD research remains open to this question.

Previous researchers have asserted that the basic problem preventing cooperation between players in the PD game is that the temptation to maximize outcomes by defecting from mutual cooperation is highly salient. If this is so, one could argue that the temptation might become even more salient if the stakes were made higher, thus leading to less, rather than more, cooperation. Clearly, an experiment systematically varying incentive size is called for.

In the experiment reported here, subjects played the PD game illustrated in Figure 1 under five incentive conditions. In one condition the numbers in the matrix represented imaginary dollars (ID). In another condition the numbers represented real dollars (RD). In the other three conditions the matrix values represented cents and were multiplied by 1, 5, and 10, respectively.

## METHOD

### *Subjects*

One-hundred subjects of both sexes were recruited by means of an advertisement placed on bulletin boards at Teachers College and in a New York weekly newspaper.[4] Money was the primary incentive used in recruitment. Ten pairs of subjects were run in each of the five experimental conditions. Pairs of subjects were assigned at random to conditions without regard to sex.

### *Procedure*

When subjects arrived at the laboratory, they were escorted to different experimental cubicles containing the game apparatus; the players never saw one another or met during the experiment. Instructions were delivered by tape recording and included the usual "individualistic orientation" instructions; that is, subjects were instructed to at-

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difference between their own earnings and those of the other player in making game decisions. Much PD research has focused on the conditions under which this kind of competitiveness is reduced or intensified.

[4] This study was conducted at two different times over a period of 2 years. Subjects in the three small real money incentive conditions (1–2, 5–10, and 10–20) were students at Teachers College. Subjects in the ID and RD conditions were recruited a year later by means of a newspaper advertisement.

tempt to maximize their own earnings without regard to the earnings of the other subject. After the instructions, subjects responded to a set of practice questions which ascertained their comprehension of the game.

Subjects played a 20-trial PD game on electric control panels which, after both players had responded, displayed the choices and outcomes of both players on each trial. Before each trial, subjects recorded what they expected the other player to do on that trial. After each trial, subjects recorded the actual choices and outcomes of both players on that trial and the earnings of both players. They did not record the cumulative earnings of either player.

Subjects in the ID conditions were given $4 in real money before the instructions were delivered. In the instructions they were told that they would keep this money regardless of the outcomes of the game. They were also given a "credit" of $10 in imaginary money and instructed to "play as though you felt real money was at stake." They were told to "feel that whether you win or lose the imaginary money is very important to you." Subjects in the RD condition were given a credit of $10 in real money and asked to attach great importance to winning or losing this money. In order to increase the credibility of the large sum of money involved, these subjects were given 10 $1 bills which they placed in their pockets at the beginning of the experiment. Subjects in the 1–2, 5–10, and 10–20 conditions were given a stake of $2 in real money before the game and, as in the RD condition, were asked to attach great importance to winning or losing this money.

At the conclusion of the game subjects were paid, carefully debriefed, and asked not to discuss the nature of the experiment with anyone.

## RESULTS

Table 1 presents the mean number of cooperative choices made by dyads in the five conditions in each of the four trial blocks. Inspection of Table 1 suggests that subjects in the ID condition were more cooperative than were subjects in the various real money conditions. Indeed, analyses of variance indicate that the game behavior of subjects in the four real money conditions was similar, and that subjects in the RD condition behaved quite differently from subjects in the ID condition.

A two-way mixed-model analysis of variance of the number of cooperative choices in the four real money conditions reveals no condition differences ($F < 1.00$), a highly significant overall decrement in cooperation over trial blocks ($F = 5.84$, $df = 3/108$, $p < .001$), and a nonsignificant Conditions × Blocks interaction effect ($F = 1.82$, $df = 9/108$, $p < .10$). Comparison of the ID and RD conditions using the same technique yields a significant condition effect ($F = 5.57$, $df = 1/18$, $p < .05$), a significant decrement in cooperation over time ($F = 16.32$, $df = 3/54$, $p < .001$), and, again, no significant Conditions × Blocks interaction effect ($F = 1.34$, $df = 3/54$, $p > .10$).

TABLE 1

MEAN NUMBER OF COOPERATIVE CHOICES MADE BY BOTH PLAYERS

| Experimental condition | Trial block | | | |
|---|---|---|---|---|
| | I | II | III | IV |
| | (1–5) | (6–10) | (11–15) | (16–20) |
| Imaginary dollars | 5.7 | 4.2 | 3.8 | 4.7 |
| $.01–$.02 | 5.0 | 4.2 | 3.7 | 2.7 |
| $.05–$.10 | 2.6 | 3.9 | 3.3 | 2.4 |
| $.10–$.20 | 3.6 | 3.3 | 2.5 | 3.2 |
| Real dollars | 4.7 | 3.1 | 2.6 | 2.2 |

Note.—The maximum number of cooperative choices in a five-trial block is, of course, 10.

Our data, then, indicate that subjects in the ID condition were choosing cooperatively about one-half (46%) of the time; this proportion is consistent with previous PD findings. As monetary incentives became strong, on the other hand, subjects chose cooperatively less frequently—about 38% of the time in the 1–2 condition and about 31% of the time in the other three. The mean total outcome of dyads in the RD condition was $−14.80, as compared with $−3 in the ID condition ($t = 2.39$, $p < .025$). Clearly, Gallo and McClintock's (1965) fears about the PD game are not borne out in this experiment.

## DISCUSSION

In view of the unambiguous results obtained in the present research, it becomes necessary to attempt to account for the markedly different results obtained by Gallo (1966) in his trucking-game experiment. A close reading of Gallo's experimental procedure does suggest some differences in the two experiments that could account for differences in their results. In Gallo's experiment, the experimenter took great pains to convince subjects in the real money conditions that they could keep all the money they earned; in his imaginary money conditions subjects were urged to play as though real money was at stake. It does not seem unreasonable to suggest, therefore, that Gallo's instructions made his subjects sensitive to the criteria by which the experimenter might judge their performance, and that these criteria might have appeared different in the two incentive conditions. The instructions in the real money conditions might have made profit maximization particularly salient to subjects as the experimenter's criterion for good performance, while the imaginary money instructions seem more ambiguous and could have allowed some of the subjects, especially those under Gallo's relative-value (competitive) instructions, to believe that they might be judged by the degree to which they earned more than their opponents.

In our experiment it was rather easy to assure subjects that they would keep what they earned, since they were publicly recruited for money; the relevant instructions were minimal. Our imaginary money instructions, however, might be accused of making profit maximization particularly salient for the subjects as the experimenter's criterion for good performance. Thus, we must be willing to admit that the differences observed between the ID condition and the RD condition could be due to artifactually inflated cooperation in the ID condition. However, the results for the ID condition are reasonably similar to those obtained in the 1–2 condition, so we do not take this possibility very seriously. Even if it were true, however, there is no evidence in our data to support the contention that subjects in the RD condition behaved *more* cooperatively than did subjects in the other conditions.

We should note that McClintock and McNeel (1966a) have also reported research which they interpret as indicating that cooperation in the PD game increases as payoff magnitude increases. Their data, however, were collected using the "Maximizing Difference Game" rather than the PD game. In this game, the mutual choice yielding the greatest joint payoff also yields the greatest payoff to each individual on any trial, and a player highly motivated to earn money has every reason to trust that his similarly motivated opponent is unlikely to defect from the most lucrative choice (which happens to be cooperative) to a less attractive one. In the Maximizing Difference Game, then, the "problem of trust" as an obstacle to achieving cooperation is alleviated, rather than exacerbated, by an increase in the strength of players' motivation to maximize their own gains.

Our results have indicated that markedly increasing payoff magnitude in the PD game does not lead to an increase in cooperation; it may be, in fact, that cooperation decreases with increasing payoff magnitude. Though the loopholes are certainly not all closed by the present results, we feel somewhat reassured about the meaning of previous research using the PD game.

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# SENSORY DEPRIVATION STRESS:

## BIRTH ORDER AND INSTRUCTIONAL SET AS INTERACTING VARIABLES[1]

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Birth order and the affective tone of orientation material interacted to produce differential anticipated stress in sensory deprivation. When the materials were reassuring, firstborns anticipated less anxiety than did later borns; when the materials were anxiety arousing, the opposite was the case. With neutral materials, no difference was found. This greater susceptibility of firstborns to set-inducing manipulations was not reflected in differential experienced stress as reported after sensory deprivation.

Since Schachter's (1959) report on the differential affiliative tendencies of anxious firstborn and later-born subjects, several investigations have attempted to demonstrate that these two groups respond differently to prospective or actual isolation. Suedfeld (1964) found that 23 of 29 male volunteers for a sensory deprivation (SD) experiment were firstborns, about the same proportion as for a social interaction experiment (Capra & Dittes, 1962), but Dohrenwend, Feldstein, Plosky, and Schmeidler (1967) had only 7 firstborns among 16 subjects, a significantly smaller proportion. Furthermore, since sex and birth order are interacting variables (Sampson, 1965), it seems necessary to compare males only with males; if this is done, the Dohrenwend et al. (1967) experiment got only 3 firstborns out of 11 male volunteers.[3] Dohrenwend and Dohrenwend (1966) reported that their firstborn group was less able to reduce its distress during the SD session than were later borns.

The Dohrenwend study (analyzed in the two references above) involved recruitment via an anxiety-arousing poster and followed up with an anxiety-arousing orientation prior to the experimental procedure. By contrast, Suedfeld's (1964) recruiting material was reassuring. Hypothesizing that the tone of the recruiting text and of the orientation may have interacted with ordinal position, Suedfeld (1968) compared the subjective stress reports of two groups of sensorially deprived subjects, one of which had received a neutral orientation and the other, a reassuring orientation. It was found that there were no ordinal position effects in the amount of stress which subjects reported after release from SD. Anticipated stress, however, showed a Birth Order × Orientation interaction: no difference between firstborns and later borns after neutral orientation, significantly lower anticipated stress among firstborns after reassuring instructions. Differences in anxiety reduction during the session occurred as a function of anticipated stress, which the Dohrenwends had not measured. Since the Dohrenwend study differed from Suedfeld's (1968) and the current experiment in a number of variables (numbers and sex of subjects, duration and mode of deprivation, rate of pay, timing and nature of the instruments), cross-experimental comparisons are at best suggestive. One such suggestion is that the Dohrenwend results were not due merely to the hypothesized interaction between birth order and isolation plus anxiety.

The current study is based upon the hypothesis that birth order interacts with the affective tone of recruiting and orientation material, in that firstborns are more conforming with such set-inducing manipulations.

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[1] This research was financed by Grant 072104 from the Rutgers University Research Council.

[2] J. V. W. Donohue contributed significantly to the design and running of the experiment, and Richard Pargament, to the analysis of the data. Others participating as research assistants were M. A. Abate, R. T. Barr, C. H. Castore, P. B. Landon, L. G. Lenard, S. E. Lisman, and Stanley Silverstein.

[3] B. S. Dohrenwend, personal communication, February 10, 1967.

Thus, positively slanted materials should attract more firstborn volunteers and induce lower anticipated anxiety in them than in later borns; negative materials should lower the proportion of firstborn subjects and lead to higher anticipated stress among those who do volunteer.

Volunteering-rate data were already available for reassuring recruiting material (79% of the all-male firstborn subjects—Suedfeld, 1964) and for anxiety-arousing material (27% of firstborn males—Dohrenwend et al., 1967). The recruiting material used in the current experiment was affectively neutral, while orientations of all three types were used.[4] A measure of social desirability set was also administered, since either high or low reported anxiety might be contaminated by differences in the perceived desirability of reporting one's feelings.

## METHOD

### *Subjects*

Seventy-eight male college students answered a newspaper advertisement requesting volunteers to "lie in a dark, silent room for 2 hours, payment $3." Since restrictions on the use of the chamber permitted only 10 subjects in each of the 6 groups, 30 firstborn and 30 later-born volunteers were randomly chosen to participate. Number of children in subjects' families ranged from one to eight; at least 6 subjects in each group came from families with three or fewer children ($Mdn = 8$).

### *Procedure*

Upon reporting for the session, subjects were given the Social Desirability Response Style Scale (Stricker, 1963), followed by one of three forms of the Subjective Stress Scale (SSS). The SSS is a Thurstone scale with 16 items (see Table 1). The subject indicates which item best describes his feelings at a specified time (see Berkun, Bialek, Kern, & Yogi, 1962; Myers, 1964; Suedfeld, 1964, 1968). The three versions used in this study presented the items in three different random orders. The first SSS was accompanied by the instruction, "Indicate how you think you'll feel while you're in the dark room."

After completing these tasks, the subject was led into the confinement chamber and instructed to remain on the bed during the entire session. The experimenter then left the chamber, and one of the three taped orientation messages (see below) was played to the subject over the intercom. When it was over, the experimenter reentered the chamber and gave the subject a second form of the SSS, asking him to indicate how he expected to feel during the session; then the experimenter left.

TABLE 1

SUBJECTIVE STRESS SCALE: STIMULI AND SCALE VALUES

| Stimulus | Value |
|---|---|
| WONDERFUL | 00 |
| FINE | 09 |
| COMFORTABLE | 17 |
| STEADY | 27 |
| DIDN'T BOTHER ME[a] | 40 |
| INDIFFERENT | 48 |
| TIMID | 57 |
| UNSTEADY | 64 |
| NERVOUS | 69 |
| WORRIED | 74 |
| UNSAFE | 76 |
| FRIGHTENED | 83 |
| TERRIBLE | 87 |
| PANICKY | 88 |
| IN AGONY | 92 |
| SCARED STIFF | 94 |

[a] On the predeprivation administrations, this item was presented as "WON'T BOTHER ME."

After 2 hours of darkness and silence in the chamber, the subject was given the third form of the SSS ("How did you feel during the session?"), a symptom checklist, and then was released. Questioning indicated that subjects in both the reassuring- and aversive-orientation groups believed that the orientation message was veridical. This was in fact the case, since the diversity of SD phenomena extends to both positive and negative effects.

### *Orientation Messages*

Three messages were used, with 10 firstborn and 10 later-born subjects receiving each. In each case, the first part of the message described the proper techniques for using the toilet, getting drinking water, and the rules of conduct for subjects (e.g., no unnecessary noise or movement, etc.). Then, each subject heard one of three messages:

> 1. Neutral. At the end of the session, we will ask you to fill out some forms and do some problems. We will ask you about your experiences while in the room, so please try to remember your feelings and activities. The period of afferent reduction begins . . . now.
>
> 2. Aversive. [Same as above, except after "try to remember your feelings and activities" insert:] We would like you to check your reactions with those of other subjects.
>
> Most subjects have reported feelings of tension and anxiety. Their ability to concentrate diminished, and their thought processes became confused and inefficient. They experienced uncontrolled free association. Frequently, unpleasant memories returned without any way to avoid them. Unpleasant sensory memories were quite common, as were varying degrees of actual hallucination.

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[4] Since the Rutgers SD laboratory has an ongoing program, we did not wish to jeopardize future recruiting by using an anxiety-arousing recruitment text.

Physical sensations included gooseflesh, itching, vertigo, extremes of heat and cold, sweating, nausea of a mild form, and occasionally loss of reference to the outside. All of these effects disappeared upon leaving the chamber.

3. Reassuring. [Same as above, except the inserted portion was] Most subjects have reported feelings of relaxation and calmness. Their ability to concentrate improved, and their thought processes became very clear and efficient. They found all forms of memory and recollection increasingly vivid; memories were frequently of very pleasant past experiences which seemed almost to have recurred. Physically, the experience has been described as soothing, comfortable, and relaxing. Some subjects report that it is as good as 8 hours of sleep.

## RESULTS

Of the 78 volunteers, 40 (51%) were firstborns, about the same proportion as was found in a random sample of the male student population (107 of 200, or 53%). There was no difference between the ordinal position groups on the Social Desirability Response Style Scale. Firstborns had a mean of 20.57; later borns, a mean of 20.56; with standard deviations of 3.37 and 2.74, respectively.

### *Subjective Stress Scale*

There was no significant difference among the groups on the first (preorientation) SSS administration; all means were around "Won't bother me," between 36.0 and 43.1 (all $Fs \leq 1.11$).

Changes in anticipated stress were evaluated by analysis of covariance of the postorientation scores, using the original SSS scores as the covariate. With all three orientation groups, no significant differences were found. Since the technique is not sensitive to the order of the means, and since the neutral group (as had been hypothesized) showed no birth-order effects—in fact, showed little pre- to postorientation change—another analysis was run eliminating that group. There was now a significant main effect for orientation ($F = 4.19$, $df = 1/35$, $p < .05$) and a significant interaction ($F = 4.88$, $df = 1/35$, $p < .05$). Table 2 shows the change scores for all groups.

Retrospective stress ratings did not differ significantly among the groups; all such ratings were lower than those on the first or second SSS. Means were in the "Comfortable" to "Steady" range. When the data were analyzed by the covariance method, there were no significant differences either with the preorientation or the postorientation ratings as the covariate, regardless of whether the analysis was performed on all instruction groups or only on two groups (all $Fs \leq 2.65$).

TABLE 2

SUBJECTIVE STRESS CHANGES

| Instruction | Subjective Stress Scale administration | | | |
|---|---|---|---|---|
| | Pre- to post-orientation | | Postorientation to postconfinement | |
| | First-borns | Later borns | First-borns | Later borns |
| Positive | −17.0 | −6.4 | − 8.3 | − 9.6 |
| Neutral | − 1.5 | 2.9 | − 5.3 | −10.9 |
| Aversive | 7.3 | −1.6 | −27.8 | −20.8 |

Note.—Negative scores indicate change toward lower stress.

### *Symptom Checklist*

The symptoms to be checked were of two kinds, positive (e.g., "Increased ability to concentrate," "Pleasant memories") and negative (e.g., "Mind wandered uncontrollably," "Nausea"). These were analyzed separately. The only significant difference was an instruction main effect on negative symptoms, $F = 6.38$, $df = 2/54$ $p < .01$, with the neutral group scoring low. The number of items checked by the experimental subjects showed no effect of either ordinal position or instruction, nor an interaction.

## DISCUSSION

It is apparent from these data that ordinal position per se does not significantly affect either one's anticipation or one's experience of SD stress. Rather, firstborn subjects are more susceptible to instructional set and tend to anticipate that degree of stress which the instructions imply will be experienced. It is to be noted that this is not merely a social desirability response. In other studies firstborns have volunteered in disproportionate numbers when the recruiting material was reassuring, but have stayed away when it was frightening; in this experiment, where it was neutral, they volunteered in proportion to their presence within the population.

Although differences of a few hours in confinement duration do not seem to affect SD phenomena (Zubek, in press), it is true that the current study used the shortest time of those studies which have investigated the role of ordinal position in the production of deprivation stress (i.e., Dohrenwend & Dohrenwend, 1966; Dohrenwend et al., 1967; Suedfeld, 1968; Zuckerman & Link, 1968). Thus, the results are not strictly comparable, although there is a consistent pattern. The present study is the only one which varied instructional set systematically. From our results, we may deduce that the major ordinal position differences in SD anxiety are actually differential preisolation anxiety levels and are induced by the recruiting and/or the orientation material. With neutral instructions and recruitment, birth order should not be a contaminating factor in volunteering for SD or in the anticipation of subjects prior to confinement.

Once again, as in previous research (Suedfeld, 1968; Zuckerman & Link, 1968), firstborn and later-born subjects were equal in the degree of actual stress as reported after SD; thus, differences in expectation did not lead to different experiences. All groups reported lower actual than anticipated anxiety. It will be noted that the difference between the postinstruction anticipation and the actual experienced stress is more than twice as great for the aversive as for the other two groups and is particularly large among aversive-instruction firstborn subjects. This finding is at variance with those of Dohrenwend and Dohrenwend (1966), who concluded that isolated firstborns have difficulty in reducing their anxiety.

The low number of negative symptoms in the neutral orientation group was unexpected. It may be that the more detailed instructions concerning symptoms sensitized the other groups and raised their reporting rates; for this or any other post hoc hypothesis, the question would be why no difference was found for positive symptoms. Since there is no validation for the symptom checklist, and since this puzzling effect was not reflected in the SSS scores, the meaningfulness of the datum must await further investigation.

These findings emphasize the role of the recruiting and orientation texts. This issue has provoked considerable controversy among SD researchers, particularly since the publication of Orne and Scheibe's (1964) article, which some workers interpreted as an indictment of the whole field. This was not the authors' intention, nor did their conclusions warrant such an interpretation; however, their work and that of others has supported the view that the "social psychology of the sensory deprivation experiment" is a noteworthy variable. In a review of the relevant studies, Suedfeld (in press) has argued that rather than considering this factor a contaminant, researchers should investigate it as they do other parameters of the deprivation situation. The current study has been one result of this suggestion.

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## *Journal of Personality and Social Psychology*

Social Learning and Piaget's Cognitive Theory of Moral Development: Philip A. Cowan,* Jonas Langer, Judith Heavenrich, and Marjorie Nathanson: Department of Psychology, University of California, Berkeley, California 94720.

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The Carryover Phenomenon: The Persistence of Reinforced Behavior Despite the Absence of a Conscious Behavioral Intention: Melvin Manis* and John Ruppe: Psychological Research Section, Veterans Administration Hospital, 2215 Fuller Road, Ann Arbor, Michigan 48105.

Responses to Cognitive Inconsistencies: Personality, Discrepancy Level, and Response Stability: David L. Hamilton*: Department of Psychology, Yale University, 333 Cedar Street, New Haven, Connecticut 06510.

Dissonance and Relative Versus Absolute Attractiveness of Decision Alternitives: Herbert J. Greenwald*: Communication Research Center, Boston University, 640 Commonwealth Avenue, Boston, Massachusetts 02215.

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# ESTHETIC PREFERENCE AND OTHER CORRELATES OF ACTIVE VERSUS PASSIVE FOOD PREFERENCE[1]

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Gombrich has suggested that esthetic sophistication may be correlated with the psychoanalytic dimension of active versus passive orality. This prediction is confirmed in a study of 105 college men, using Wolowitz's measure of food preference and Child's measure of esthetic sensitivity. Each of these two measures is found to be related to active versus passive preferences in several other spheres of life, and to several measures of independence versus dependence. Neither oral nor esthetic preference, on the other hand, appears to be related to measures of achievement, dominance, and aggression. These findings have no clear implication for the causal priority of one or another of the variables found to be interrelated.

The art historian, E. H. Gombrich (1953), in considering the usefulness of psychoanalytic theory in understanding response to art, has suggested both a parallel and a correlation between food preferences and art preferences.

> My guess is, for instance, that small children and unsophisticated grown-ups will be likely to enjoy a soft milk chocolate, while townified highbrows will find it cloying and seek escape in the more bitter tang or in an admixture of coffee or, preferably, of crunchy nuts. [p. 39]

One of the authors (Child, 1965) has developed a measure of "esthetic sensitivity" which seems directly pertinent to the contrast between unsophisticated and highbrow response to art latent in this quotation and explicit in Gombrich's accompanying discussion. Another (Wolowitz, 1964) has developed an inventory of food preferences to measure the dimension of active versus passive food preference which Gombrich drew from psychoanalysis. Applying these two measures to a single group of subjects makes possible a test of the relationship Gombrich suggests, between esthetic sophistication in art and preference for activity and challenge in the use of the mouth for eating. We report here the results of such a test.

At the same time we also report the relation of each of the above two measures to a number of other personality variables. Here we are testing the extension to new variables of earlier findings on correlates of food preference (Wolowitz, 1964, 1967) and on correlates of esthetic sensitivity (Child, 1965; Child & Iwao, 1968). Most of the new variables were measured to test a definite prediction that they should be either positively or negatively related to active food preference (as indicated, respectively, by a plus or minus in the first column of Table 1); reasons for the prediction will be stated as the variables are described. The rest of the new variables, for which we had no prediction, were measured for purposes of other research being done simultaneously with the same subjects and are presented (carrying a 0 in the first column) below the others for comparison. If esthetic preference or sensitivity has much in common with active food preference, the same pattern of correlations with other variables might be predicted for both. Other grounds readily appear, too, for predicting a correlation of esthetic preference with some of the other variables, and we will also try to state these grounds as we describe the variables.

## GENERAL PROCEDURE

The subjects were 105 male undergraduates at Yale University, who responded to a bulletin-board

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TABLE 1

PERSONALITY SCALES AND THEIR CORRELATIONS WITH ACTIVE FOOD PREFERENCE AND ESTHETIC PREFERENCE FOR 105 UNDERGRADUATE MEN

| Variable | Direction of prediction[a] | No. items | | Alpha coefficient | Correlation with | | Loading on first unrotated factor |
|---|---|---|---|---|---|---|---|
| | | Eliminated | Kept | | "Active" food preference[b] | Esthetic preference[b] | |
| A. Preference for | | | | | | | |
| "Active" food | + | 13 | 22 | .682 | | .220* | .463 |
| Esthetically better art | + | 14 | 24 | .782 | .220* | | .504 |
| B. Preference for | | | | | | | |
| Intellectual challenge | + | 6 | 19 | .685 | .225** | .221* | .464 |
| Sensory challenge | + | 6 | 20 | .641 | .226** | .246** | .586 |
| Sexual challenge | + | 6 | 7 | .584 | .120 | .100 | .418 |
| Decision making | + | 3 | 11 | .592 | .198* | .298** | .641 |
| Vigorous action | + | 5 | 10 | .500 | .160 | .022 | .366 |
| Work | + | 2 | 12 | .593 | .318** | .218* | .591 |
| C. Tendency toward | | | | | | | |
| Achievement | + | 0 | 10 | .780 | .003 | (−.172) | −.191 |
| Achievement anxiety | − | 1 | 9 | .578 | (.003) | (.261) | .070 |
| Dominance | + | 0 | 10 | .689 | .028 | (−.000) | −.093 |
| Dominance anxiety | − | 3 | 7 | .633 | (.107) | (.033) | .035 |
| Aggression | + | 0 | 10 | .628 | (−.067) | .009 | .109 |
| Aggression anxiety | − | 1 | 9 | .632 | −.144 | −.077 | −.382 |
| D. Tendency toward | | | | | | | |
| Autonomy | + | 2 | 8 | .580 | .053 | .080 | .327 |
| Autonomy anxiety | − | 1 | 9 | .728 | −.255** | −.258** | −.620 |
| Dependence | − | 0 | 10 | .655 | −.144 | −.174* | −.509 |
| Dependence anxiety | + | 0 | 10 | .698 | .242** | .148 | .444 |
| Deference | − | 2 | 8 | .591 | −.042 | −.311** | −.508 |
| Deference anxiety | + | 1 | 9 | .587 | .240** | .209* | .471 |
| Independence of judgment | + | 6 | 16 | .726 | .274** | .453** | .721 |
| E. Tendency toward | | | | | | | |
| Affiliation (Sociability) | 0 | 1 | 9 | .713 | −.213* | −.188 | −.372 |
| Affiliation anxiety | 0 | 1 | 9 | .507 | .060 | −.010 | .041 |
| Isolation | 0 | 0 | 10 | .592 | .061 | .122 | .200 |
| Isolation anxiety | 0 | 0 | 10 | .695 | −.221* | −.151 | −.512 |
| Nurturance | 0 | 1 | 9 | .725 | −.060 | −.157 | −.242 |
| Nurturance anxiety | 0 | 0 | 10 | .748 | .167 | .094 | .121 |
| Extroversion | 0 | 3 | 21 | .844 | −.032 | .048 | .078 |
| Anxiety | 0 | 0 | 24 | .849 | −.091 | .121 | −.094 |
| Sexual guilt | 0 | 0 | 14 | .802 | −.181 | −.138 | −.420 |

[a] + indicates prediction of a positive correlation with active food preference and esthetic preference; − indicates prediction of a negative correlation with active food preference and esthetic preference; 0 indicates no prediction made.

[b] ( ) indicates the correlation is opposite to prediction.

* $p < .05$ (two-tailed test for variables in Section E, one-tailed for others).

** $p < .01$ (one-tailed test).

request for paid subjects for a study of personality and preferences.

Each subject was given two questionnaires: the food-preferences index and a three-part questionnaire to measure other personality variables. Interspersed with work on the questionnaires were three periods of responding to pairs of pictures projected on a screen. The picture pairs were mostly presented as part of the other study being conducted simultaneously, but some provided the measure of esthetic sensitivity used here.

Testing was done in group sessions, with each subject seated in a position that would afford him complete privacy while filling out the questionnaires. Subjects were asked to be honest in their answers and were assured of complete anonymity. They were to mark all items; if, in rare instances, they had not experienced the situation referred to, they were asked to imagine it as best they could and answer accordingly.

The specific variables measured are listed in Table 1 and will be described in later sections. The following procedure was used to assess and to improve internal consistency: For each variable separately, the items were intercorrelated and factor analysis was applied to the resulting matrix. All items were eliminated whose loading on the first unrotated factor was less than .20 and also, for one variable, a single item with a loading larger than .20 in the unexpected direction. Table 1 shows in the second and third columns the number of items eliminated and the number kept. Subjects were then scored on the variable, and the internal consistency of the scores was measured by the alpha coefficient, which is the average of all possible split-half correlation coefficients. The alpha coefficient for each variable is presented in the fourth column of Table 1.

## RESULTS

### *Measurement of Active Food Preference*

The year before this research was done, Michael Straight, Jr. (unpublished research), gave to 68 Yale undergraduates a 108-item form of the Wolowitz inventory. In his data, 35 of the items had a correlation of .30 or more with total score. We used only these 35 items, and in our factor analysis 22 had first-factor loadings of .20 or more. Because of the central importance of this measure, we give the items in full, in descending order of loading. The oral-active choice always appears first here, and the oral-passive second (we will use a comparable order for other kinds of items), though in the inventory, of course, the order was reversed for about half the items. The items are: bittersweet chocolate versus sweet chocolate; salami versus bologna; barely ripe fresh fruit versus ripe fresh fruit; sour apple versus sweet apple; dark bread versus light bread; unsweetened juice versus sweetened juice; sour cherries versus sweet cherries; hard candy versus soft candy; crust of the bread versus middle of the bread; lemonade versus orangeade; sour pickles versus sweet pickles; lemonade versus coke; raw pineapple versus canned pineapple; lemon wafers versus chocolate-chip wafers; sharp cheese versus cream cheese; hard roll versus soft roll; sauerkraut versus cooked cabbage; fresh peaches versus canned peaches; cookies versus cake; cherry pie versus cream pie; whole cranberry sauce versus cranberry jelly; tomato soup versus cream of tomato soup.

These items seem to represent rather well the following characteristics among the ones proposed by Wolowitz for active food preferences: less sweet, more sour, harder or more solid, more highly seasoned or strong flavored. Less well represented are: less rich, saltier, more bitter, drier, and thicker in consistency.

### *Preference for Esthetically Better Art*

This variable was measured by having the subject indicate which picture he liked better, for each of 38 pairs of pictures. The two pictures in a pair were similar in subject matter and generally in style; out of each pair experts had shown good agreement in choosing one work as esthetically better (though the subject was not told so). From over 1,000 such pairs available from earlier research the 38 were selected for usefulness of their subject matter to the simultaneous study. Responses to each pair were scored for agreement with expert judgment of esthetic merit. In a factor analysis of the interitem correlations, 24 of the 38 pairs had first-factor loadings of .20 or more, and a person's score for esthetic preference was the number of pairs out of these 24 on which his preference coincided with expert judgment.

Gombrich's prediction is confirmed by a significant correlation of .220 between this measure and active food preference. Of the 22 separate food choices, only 4 have negative correlations, averaging −.05, with esthetic preference, while the other 18 have positive correlations, averaging .11.

The correlation between esthetic preference and food preference would be expected to be higher if the two variables were measured more reliably, as they could be by using many more items. We may ask what estimate present evidence would yield about the size of the correlation if measurement could be completely reliable. Correction for attenuation is a statistical procedure enabling us to answer that question (see, for example, Lord & Novick, 1968, pp. 69–71). The corrected correlation is .301. Since the proportion of variance of one variable associated with another variable is given by the square of their correlation coefficient, we arrive at the estimate that these two variables, if measured perfectly reliably, would share about 9% of their variance. There is no suggestion, then, that the two variables are very close to each other; yet even so much of a relationship may well surprise us when the manifest nature of the two variables differs so radically.

### *Preference for Activity in Spheres other than Oral and Artistic*

Regardless of whether active food preference reflects in orality a more general preference for activity over passivity, or whether it is one symptom of a trait originating in orality and then generalized beyond, we would expect to find it positively related to preference for activity in other spheres as well. The correlation we have just reported between active food preference and esthetic preference may be regarded as one confirmation of this expectation. The view that esthetic sensitivity is one possible expression of an active cognitive orientation (Child, 1965) does not, of course, have to depend upon any reasoning relating to orality, and thus we would expect —quite independently of any theory about orality—that esthetic sensitivity might be positively related to activity in a variety of other spheres. We developed, especially for this study, items intended to measure six variables of preference for activity in areas other than oral and artistic.

*Preference for intellectual challenge.* This variable was measured by items posing a choice between alternatives distinguished in degree of intellectual challenge they offer, or asking about liking for a possibility either very low or very high in intellectual challenge. Items were in the form of questions to be answered by checking Yes or No. The 3 items with the highest loading on the first factor are: Does knowing that something will be very hard to understand make it more interesting to you? (Yes) Would you rather spend an hour solving several easy math problems than solving one hard one (No) Do you get a kick out of trying to solve a puzzle. even when you fail? (Yes)

*Preference for sensory challenge.* This variable was defined as a liking for arousing, assaultive, or invigorating rather than quieting or relaxing stimulation. The three items with the highest loadings were: Swimming in a heated pool in winter, would you rather have the pool outside in the frosty air, or inside a heated building? Sleeping in the winter, do you prefer a chilly bedroom, or a comfortably warm bedroom? Do you prefer sleeping on a hard mattress, or on a soft mattress?

*Preference for sexual challenge.* This variable was intended to sample a number of aspects of preference for challenge in relations with the opposite sex. One aspect—relative interest in the genital area of the female body rather than in the breast area with its possible maternal significance continued from early childhood—was perhaps represented by too many items, and the three with highest loadings on the first factor (and one other among the seven retained) refer to it. We append to them the fourth item as an example of those referring to other aspects of sexual challenge: Which do you find more exciting—seeing an attractive girl in a tight-fitting skirt, or in a tight-fitting sweater? Do you find yourself generally more attracted by the appearance of girls' hips, or breasts? Which do you find more exciting on an attractive girl—a very short skirt, or a low-cut evening gown? Do you find more interesting a girl who obviously admires other men, too, or one who seems to admire you alone?

*Preference for decision making.* These items attempted to sample a variety of situations where decisions might be made by the person himself or by others. The three most representative items are: Do you prefer deciding what is morally right yourself, or accepting the moral code of society or institution? Do you believe in individualized religion, or strictly institutionalized religion? Would you prefer a job that required making difficult decisions, or following instructions carefully?

*Preference for vigorous action.* Here, various contrasts were offered between a more vigorous act and a less vigorous one which in at least some other respects would serve similar ends: Would you generally prefer driving fast, or slow? Would you rather spend an hour breaking rocks with a pick, or shoveling sand? Would you rather swim in the surf, or in a calm bay?

*Preference for work.* The contrast intended here is between more and less total work involved in doing something (rather than, as above, in the vigor or rapidity of each specific act): Would you rather open a heavy door yourself, or have it open automatically? Do you prefer to climb a flight of stairs, or take an elevator up? Would you rather cut down weeds with a scythe, or a mowing machine?

The prediction that active food preference would be positively related to active preference in these various other spheres is amply confirmed, as indicated in Section B of Table 1. The correlations are uniformly positive, and several are highly significant. For esthetic preference, similar confirmation is found except that one correlation is near zero. When we consider separately the six measures grouped here, we find that only for sexual challenge and vigorous action are the correlations too low to conclude that the measure clearly yields significant results. The other four variables—preference for intellectual and sensory challenge, decision making, and work—are significantly correlated both with active food preference and with art preference.

An overall test of this prediction can be made by pooling the items of all six specific scales on preference for activity. The results are given in the first line of Table 2. The composite scale has correlations significant well beyond the .01 level both with active food preference and with esthetic preference. Corrected for attenuation, the correlations are .368 and .376, respectively, yielding in each instance the estimate that with perfectly reliable measures about 14% of the variance would be associated with preference for activity.

### *Tendency to Strive against Social Obstacles*

The personality variables in the next group (C in Table 1) have in common an active or counteractive orientation toward other people as obstacles, competitors, or manipulanda. Other people are not mentioned in all statements, but they are always potentially relevant. It seems possible that such an orientation in social interaction might be related to active food preference; and of all our variables, these are the ones for which a relationship would be most specifically predicted from the label of *oral sadism* which many writers have applied to active orality. There seems to be no convincing separate reason to expect esthetic preference to be correlated positively with them.

TABLE 2

COMPOSITE PERSONALITY SCALES: CORRELATIONS WITH ACTIVE FOOD PREFERENCE AND ESTHETIC PREFERENCE FOR 105 UNDERGRADUATE MEN

| Composite variable | No. items | Corrected split-half reliability | Correlation obtained with | |
|---|---|---|---|---|
| | | | Food preference | Esthetic preference |
| Preference for activity | 79 | .863 | .282 | .309 |
| Striving against social obstacles | 55 | .810 | −.002 | −.118 |
| Independence | 70 | .874 | .312 | .402 |
| Positive social relations | 57 | .868 | −.202 | −.186 |

This group of variables, like the ones in the other two groups still to be considered, were not measured by obtaining expressions of preference. The items simply consisted of statements to which the subject responded by indicating whether they did or did not hold true of himself. The variables are in pairs: corresponding to each behavior tendency is a variable representing anxiety about or avoidance of it. The statements were all phrased positively; that is, acceptance of the statement as self-descriptive scored a positive point on the variable the statement is intended to measure. The names of the variables are clear enough so that we will give no definition other than citing the three statements with the highest loading on the first factor.

*Achievement*. Ambition drives me to ever-greater efforts. Above all I want to have an outstandingly successful career. I find it very hard to reconcile myself to losing out in any competition.

*Achievement anxiety*. I'd rather live a quiet and modest life than keep striving for advancement. I feel that in our schools too much stress is put on achievement. I dislike the discipline required for successfully accomplishing some difficult task.

*Dominance*. I enjoy organizing or directing the activities of a group, team, club, or committee. I would like to be chairman of an organization and take a firm lead in directing its activities. A job I would like very much is an executive post which would permit me to organize and direct the work of others.

*Dominance anxiety*. I feel inadequate when I have to supervise other people's work. I dislike having to make plans which other people will have to follow. One thing I would dislike about being a military officer is having to order people around.

*Aggression*. I generally find that attack is the best way to defend myself. When somebody offends me, my greatest desire is to get even. I am apt to enjoy getting a person's goat.

*Aggression anxiety.* If there is one thing that makes me feel ill at ease, it's an uncontrolled and passionate clash of people of different ideas. When I realize I have hurt somebody's feelings, I always feel guilty about it. I hate any form of argument and will go out of my way to avoid it.

The relationships predicted here are definitely not confirmed. The correlations are generally small, half of them are in a direction opposite to prediction, and none of those in the predicted direction are of sufficient magnitude to be considered statistically significant. The absence of confirmation appears also when the items of these six specific scales are pooled to form a composite scale of tendency to strive against social obstacles. As indicated in Table 2, this composite variable is not significantly correlated with either food preference or esthetic preference; while one of the correlations is almost zero, both are in the direction opposite to prediction.

### *Tendency toward Independence*

Seven different variables can be grouped under the general heading of independence (D in Table 1). Six are similar to the ones we have just been considering, in consisting of modes of human interaction and of anxiety about or avoidance of each. These six can be sufficiently defined by the three most representative items. The seventh variable, independence of judgment, requires more description.

*Autonomy.* I usually tend to do my planning alone, without suggestions from, or discussion with, other people. I demand independence and liberty above anything else. I usually arrive at decisions without talking the problem over with others.

*Autonomy anxiety.* I feel very insecure about reaching any decision of importance entirely on my own. I am afraid of situations where I have to rely entirely on my own judgment. If I were at the head of a business or other enterprise, what would make me most uncomfortable is the occasional necessity for making decisions entirely on my own.

*Dependence.* I enjoy the realization that I know one or two older people whose wisdom and sympathy I can rely on. When I have problems and doubts concerning my studies, I often wish for somebody who could push me in the right direction. Often when I have become confused, others have helped me find the right way out.

*Dependence anxiety.* I can't stand it when people get concerned with my problems and offer all kinds of advice and assistance. When I am unhappy, it makes me feel worse to have people express sympathy. When I have any trouble or difficulties, it makes me feel uncomfortable to have to appeal to someone who is in a position to help me.

*Deference.* I conform to conventions even when I don't like them. I readily abandon personal plans of my own when I find that my friends disapprove of them. I tend to agree with my friends on most important issues.

*Deference anxiety.* The thing I would particularly hate about military service is the requirement of obeying all orders of my immediate superior. I am unable to work efficiently when I am in a subservient position. I resent anybody whose position is superior to mine and who can exert a certain authority over me.

*Independence of judgment.* This is derived from the scale devised by Barron (1953b). He prepared many questionnaire items which on various grounds might have been expected to be correlated with maintaining individual judgment versus yielding to the judgment of others, in Asch's social pressure experiment. Barron's scale consists of those items which he found in fact to be significantly correlated with this criterion. The five we found to have the highest first-factor loadings are: If young people get rebellious ideas, then as they grow up they ought to get over them and settle down. (No) What the youth needs most is strict discipline, rugged determination, and the will to work and fight for family and country. (No) It is easy for me to take orders and do what I am told. (No) The best theory is the one that has the best practical applications. (No) Some of my friends think that my ideas are impractical, if not a bit wild. (Yes)

The correlations of these seven measures of tendency toward independence, both with active food preference and with esthetic preference, are uniformly in the predicted direction. Half of them reach well beyond the .01 level of statistical significance, and one—the correlation between esthetic preference and independence of judgment—has the remarkably high value of .453. Though perfectly consistent in direction, the correlations are in some instances very low; one of the variables (autonomy) has no significant correlations, and another (dependence) has no very substantial correlation. But the other five out of the seven variables have a very significant and substantial correlation with either food preference or esthetic preference or both.

The composite variable formed by pooling the 70 items from these various independence scales has highly significant correlations both with active food preference and with esthetic preference, as indicated in Table 2. Applying the correction for attenuation yields correlations of .404 and .486, leading to the estimate that with perfectly reliable measures 16% of the variance in food preference and 24% of the variance in esthetic preference would be associated with independence tendency. Here the indication is of a very substantial relation.

### *Other Variables*

For purposes of the other research being done simultaneously, nine additional personality variables were measured. They appear in Section E of Table 1. Because of the lesser relevance of these variables, we will not give illustrative items. Six of the variables referred to three modes of human interaction and corresponding anxieties: affiliation, isolation, and nurturance (i.e., aiding the helpless or dependent). Extroversion and general anxiety were measured by items from the Maudsley Personality Inventory (Eysenck, 1956); the variable we term "anxiety" here is what Eysenck calls "neuroticism." Sexual guilt was measured with items assembled by Child (1965).

The correlations with food preference and esthetic preference are much lower here where we had made no prediction than in the groups (except C) for which we had made a prediction. Two of the nine variables here (affiliation, and isolation anxiety) do show with active food preference correlations—negative in both instances—significant at the .05 level. It seems to us after knowing these correlations that their direction might have been predicted from thinking about the passive end of the food-preference dimension. The passive dependence suggested by the "inactive" food preferences seems conducive to affiliative interests and to anxiety about being alone.

We may follow up this lead by forming a composite measure of positive social relations, by adding a person's scores on affiliation, isolation anxiety, and nurturance, and subtracting the sum of his scores on affiliation anxiety, isolation, and nurturance anxiety. (We omit from the composite three variables in this group—extroversion, anxiety, and sexual guilt—which do not seem so clearly relevant to what we intend by "positive social relations.") This composite, as shown in Table 2, yields low but suggestive correlations with food preference and esthetic preference. The one with esthetic preference is large enough to be significant at the .05 level. We must be cautious about concluding it is significant, since it is based on a post hoc assembly of scales; but the suggested relationship may be worth considering in later research.

### *Factor Analysis*

To explore the pattern of relationships among our 30 variables, we have done a principal components factor analysis on the intercorrelations among them. The first factor is an axis so located as to gather together the greatest amount possible of the shared variance. The loading of each variable on this factor measures the extent to which that variable shares in the general tendency so defined. These loadings appear in the last column of Table 1.

Let us take .30 as the boundary between high and low loadings on this first factor. (This arbitrary value gives an especially neat picture; other reasonable values give pictures of similar import though more complicated.) We are not surprised to find the high loadings concentrated among the scales of independence and preference for activity. Indeed, high loadings characterize all 13 scales in those two categories, but only one of the 6 scales for striving against social obstacles and only three of the 9 scales in the miscellaneous group. High loadings also characterize the two scales on which our interest is mainly focused, esthetic preference and food preference. They rank ninth and twelfth among the 30 variables, in size of first-factor loading. In size of loading they are right up with some of the questionnaire measures of independence and activity-preference, despite sharing little if any of the manifest resemblance that can be found among many of the questionnaire measures. This initial view of the factor analysis, then, supports the inference that may be drawn from the earlier results, that esthetic preference and active food preference are part of a general pattern of liking to be independent and to be active in confronting, on one's own, the challenges life offers.

This general factor accounts, however, for only 17% of the total variance. A second factor accounts for 13%, and a third for 10%, and altogether there are nine factors which each account for at least as much variance as each of the original scales (i.e., 3.3% of the total variance, since there are 30 scales and the variances of all are given equal weight). These nine factors have been rotated to the varimax criterion in an attempt to define some of the less general patterns of relation among variables. The rotated factors are presented in Table 3, arranged approximately in descending order of importance for understanding food and esthetic preference.

If we again take .30 as defining a high loading, there are only three factors on which

TABLE 3

ROTATED FACTOR MATRIX: LOADINGS OF 30 VARIABLES ON 9 FACTORS

| Variable | Factor, label, and % variance | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | I Independence 8.2% | II Food Preference 4.7% | III Anti-achievement 6.2% | IV Liking for Effort 8.2% | V Fear of Self-assertion 6.3% | VI Introversion 10.0% | VII Anxiety 7.1% | VIII Misanthropy 9.8% | IX Superego 6.1% |
| A. Preference for | | | | | | | | | |
| "Active" food | .11 | .69 | −.07 | .23 | .01 | .17 | −.13 | .14 | −.01 |
| Esthetically better art | .53 | .35 | .32 | −.07 | −.20 | .14 | .14 | .05 | .04 |
| B. Preference for | | | | | | | | | |
| Intellectual challenge | .50 | .18 | −.32 | .43 | −.09 | −.11 | −.04 | −.06 | .17 |
| Sensory challenge | .13 | .04 | .32 | .72 | −.22 | .08 | −.09 | −.04 | −.07 |
| Sexual challenge | .43 | −.19 | .22 | .37 | .20 | .08 | −.06 | .06 | .06 |
| Decision making | .54 | .25 | .04 | .42 | −.28 | −.14 | .00 | −.20 | −.22 |
| Vigorous action | .00 | .12 | −.23 | .68 | −.02 | −.08 | −.08 | .05 | −.10 |
| Work | .24 | .12 | .05 | .68 | .06 | .23 | .12 | −.08 | −.04 |
| C. Tendency toward | | | | | | | | | |
| Achievement | −.09 | .08 | −.70 | .04 | −.12 | −.25 | .16 | .18 | .24 |
| Achievement anxiety | .10 | .15 | .69 | .04 | −.03 | −.21 | .40 | .03 | .21 |
| Dominance | .09 | .19 | −.49 | −.22 | −.51 | −.34 | .17 | −.10 | −.08 |
| Dominance anxiety | −.00 | .12 | .15 | .13 | .53 | .09 | .32 | .36 | .23 |
| Aggression | .08 | −.20 | −.27 | .03 | −.13 | −.19 | .43 | .36 | −.51 |
| Aggression anxiety | −.16 | .04 | −.03 | −.03 | .11 | −.19 | .17 | .08 | .84 |
| D. Tendency toward | | | | | | | | | |
| Autonomy | .00 | −.00 | −.04 | .23 | −.65 | .01 | .32 | .33 | −.05 |
| Autonomy anxiety | −.34 | −.27 | −.13 | −.18 | .51 | −.18 | .36 | −.02 | .14 |
| Dependence | −.22 | .05 | .06 | −.06 | .20 | −.62 | .38 | −.33 | .02 |
| Dependence anxiety | .12 | −.03 | −.07 | .24 | −.32 | .35 | −.02 | .57 | .24 |
| Deference | −.80 | .19 | −.01 | −.01 | .08 | −.08 | .18 | .01 | .05 |
| Deference anxiety | .43 | .30 | −.01 | .15 | .10 | −.20 | .21 | .56 | −.17 |
| Independence of judgment | .74 | .25 | .02 | .23 | .02 | .14 | .15 | −.01 | −.22 |
| E. Tendency toward | | | | | | | | | |
| Affiliation | −.02 | −.24 | −.16 | −.03 | −.17 | −.77 | −.02 | −.10 | −.00 |
| Affiliation anxiety | −.08 | .03 | −.20 | −.10 | .04 | .11 | .24 | .68 | .03 |
| Isolation | .03 | −.05 | −.04 | .01 | −.01 | .53 | .51 | .28 | .12 |
| Isolation anxiety | −.15 | .04 | −.03 | −.13 | .10 | −.79 | .09 | −.03 | .19 |
| Nurturance | −.01 | .04 | −.21 | .10 | −.14 | −.36 | .14 | −.64 | .14 |
| Nurturance anxiety | −.10 | .09 | .00 | −.07 | −.07 | .04 | .04 | .86 | .07 |
| Extroversion | .23 | −.18 | −.06 | .15 | −.42 | −.56 | −.19 | −.07 | −.11 |
| Anxiety | −.02 | −.04 | .05 | −.06 | −.01 | −.04 | .82 | .09 | .01 |
| Sexual guilt | −.01 | −.40 | −.11 | −.33 | .09 | .07 | −.06 | .04 | .60 |

either food or esthetic preference shows a high loading. These are:

I. A factor we may call Independence, on which the variables with high loading are as follows: deference, —.80; independence of judgment, .74; decision making, .54; esthetic preference, .53; intellectual challenge, .50; deference anxiety, .43; sexual challenge, .43; autonomy anxiety, −.34. This factor places esthetic preference rather high among a set of variables defining a liking for being one's own master. It confirms the paradoxical finding of previous research (Child, 1965; Child & Iwao, 1968) that esthetic preference, while defined by agreement with experts, characterizes people who like to make up their own minds rather than deferring to anyone else. On this factor, food preference has a loading of only .11. Joint participation in an Independence factor might be a stable, even though small, part of the linkage between food preference and esthetic preference, but the possibility of sampling error prevents our drawing any definite conclusion.

II. A highly specific factor, the only one on which Food Preference has a high loading. Its loading is .69, and that of esthetic preference is .35. The only other variable with a high loading is sexual guilt (negative loading). This factor links the two variables of special interest, and suggests that their linkage may be partly independent of the general patterns with which the previous sections of the paper show each of them to be associated.

III. An Antiachievement factor, on which the highest loadings are for achievement (−.70), achievement anxiety (.69), and dominance (−.49). Esthetic preference has a loading of .32, and this factor links it also with liking for sensory challenge (.32) rather than intellectual challenge (−.32). A slightly smaller loading of aggression (−.27) may be joined with all these to suggest that the factor resembles hippie ideology. The contemplative and antipractical aspect of esthetic orientation is what seems pertinent here. Food preference has no obvious relevance. In the hippie movement, or in its personality origins, we may then have a source of esthetic preference responsible for its unexpected relation to anxiety about achievement, but not relevant to the main body of the findings we have reviewed.

From some of the other factors, very tentative inferences might be drawn about influences on our two central variables, and to make it easier to do so we have given each factor a label. The inferences would be so uncertain, however, that we will not discuss them here.

The unrotated factor analysis, then, shows that esthetic preference and active food preference both enter very markedly into the main pattern of correlation among variables to be found in our data. The rotated factor matrix shows that esthetic preference and active food preference may be partly linked through specific connections not related to their separate association with the other variables studied, that esthetic preference is part of a pattern of variables that may be called Independence, but that it is also to a lesser extent part of a pattern of variables that may be called Antiachievement.

## DISCUSSION

We have evaluated Gombrich's suggestion that the quality of an individual's visual esthetic responsivity is associated with position on the dimension conceptualized by psychoanalytic theory as active versus passive orality. Our test of the hypothesis yielded, specifically, a small but significant correlation between active choices on a food-preference inventory and an index of visual esthetic sensitivity based on extent of agreement between personal preference and expert judgment of esthetic value. Our data yield a prediction that with perfectly reliable measurement these two variables would share 9% of their variance. Subsequent research (Cooperman & Child, 1968) has confirmed this relation in 172 subjects drawn from a similar population, with a higher correlation between food preference and esthetic sensitivity.

This evidence, though consistent with Gombrich's hypothesis which led us to it, does not necessarily argue for the causal priority which psychoanalytic theory would give to the orality. We would not argue that esthetic sensitivity deserves priority. But an individual's status on both these variables may represent his position on a more general dimension of active versus passive orientation, determined by influences having little or nothing to do with orality in particular. We have indeed found evidence for a more general dimension. Active versus passive manifestations in oral and esthetic realms are related to degree of activity or challenge preferred in other aspects of behavior or of stimulation.

One of our findings could provide a very tentative argument against giving a dominant position, within this general dimension, to somatic or "psychosexual" aspects. Our variable of sexual challenge did not show very significant results, whereas variables much more remote from bodily concerns did. Against this fact, however, we would place one outcome of the factor analysis: after the axes were rotated, esthetic preference and active food preference shared high loadings only on a factor in which they were clearly joined with a specifically sexual variable, absence of sexual guilt; this three-way union suggests a possible relevance of Freud's theory of psychosexual development.

Whatever the ultimate explanation of our findings, they are consistent with, and extend the generality of, previous findings on each of the separate preferences we have concentrated on: food preferences and esthetic preferences. Our findings on food preferences confirm, with a variety of new variables, Wolowitz's (1965) finding that passive food preference is related to questionnaire measures compatible with the Freudian picture of passive oral character. In providing this confirmation, our findings also further support Wolowitz's view that the relation of passive food preferences to alcoholism and ulcers is relevant to the psychodynamic interpretation of those conditions as arising from conflicts about passivity or dependence.

Our findings on esthetic preference confirm previous evidence that an esthetic orientation toward art is related to an active cognitive approach to experience in general (Child, 1965; Child & Iwao, 1968). Since an esthetic orientation very often—though not always—involves preferring the complex to the simple, our findings are also consistent with Barron's (1953a) finding that preference for complex art is related to such traits as originality and liking for change, and to many of the items in his Independence of Judgment scale.

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# TERRITORIAL DEFENSE AND THE GOOD NEIGHBOR

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A series of questionnaire and experimental studies was designed to explore how people mark out and defend space in public areas. The use of space is affected by instructions to defend actively the area or retreat, by room density, and by the location of walls, doors, and other physical barriers. Under light population pressure, most markers are capable of reserving space in a public area, but more personal markers have the greatest effect. As room density increases, the effect of the marker is seen in delaying occupancy of the area and in holding onto a smaller subarea within the larger space. Neighbors play an important part in legitimizing a system of space ownership.

The concept of human territoriality is receiving increased attention. In addition to the popular books by Ardrey (1961, 1966), a number of social scientists have become impressed with the utility of the concept (Altman & Haythorn, 1967; Esser et al., 1965; Hall, 1966; Lipman, 1967; Lyman & Scott, 1967). Hediger (1950) defined a territory as "an area which is first rendered distinctive by its owner in a particular way and, secondly, is defended by the owner." When the term is used by social scientists to refer to human behavior, there is no implication that the underlying mechanisms are identical to those described in animal research. The major components of Hediger's definition are *personalization* and *defense*. Roos (1968) uses the term *range* as the total area an individual traverses, *territory* as the area he defends, *core area* as the area he preponderantly occupies, and *home* as the area in which he sleeps. Goffman (1963) makes the further distinction between a territory and a *jurisdiction*, such as that exercised by a janitor sweeping the floor of an office and keeping other people away. Territories are defended on two grounds, "you keep off" and "this space is mine." Jurisdictions are controlled only on the former ground; no claim of ownership, no matter how transitory, is made.

In a previous study, the reactions to staged spatial invasions were investigated (Felipe & Sommer, 1966). There was no single reaction to a person coming too close; some people averted their heads and placed an elbow between themselves and the intruder, others treated him as a nonperson, while still others left the area when he came too close. The range of defensive gestures, postures, and acts suggested that a systematic study of defensive procedures would contribute materially to our knowledge of human spatial behavior. Following the tradition of ecological research, the studies would be undertaken in naturally occurring environments.

## QUESTIONNAIRE STUDIES

During previous observations of library study halls Sommer (1967) was impressed by the heavy concentration of readers at the side-end chairs. Interviewing made it clear that students believed that it was polite to sit at an end chair. Someone who sat, for example, at a center chair of an empty six-chair table (three chairs on each side) was considered to be "hogging the table." There appeared to be two styles by which students gained privacy in the library areas. One method was avoidance, to sit as far away from other people as one could. The other method was offensive ownership of the entire area. To study the two methods of gaining privacy, a brief questionnaire was constructed which presented the student with table diagrams containing 6, 8, and 10 chairs, respectively (Sommer, 1967). Two forms to the questionnaire were distributed randomly within a class of 45 students. Twenty-four students received avoidance instructions: "If you wanted to be as far as possible from the distraction of other people, where would you sit at the table?" Twenty-one other students in the same class were shown the same diagrams and given the offensive display instructions: "If you wanted to have the table to yourself, where would you sit to discourage anyone else from occupying it?" Even though both sets of instructions were aimed at insuring privacy, the two tactics produced a striking difference in seats chosen. Those students who wanted to sit by themselves as far as possible from other people overwhelmingly chose the *end* chairs at the table, while those students who wanted to keep other people away from the table almost unanimously chose the *middle* chair.

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[1] The authors are grateful to Harriet Becker, Martha Connell, Ann Gibbs, Lee Mohr, Tighe O'Hanrahan, Pamela Pearce, Ralph Requa, Sally Robison, and Nancy Russo for their assistance. Requests for reprints should be sent to Robert Sommer, Department of Psychology, University of California, Davis, California 95616.

When the findings were discussed with architect James Marston Fitch, his first question concerned the location of the door in regards to the table. This seemed a good question, since the preferred location for retreat or active defense should be guided by the path the invaders would take or by the most accessible escape route. The previous diagrams had depicted only a table and chairs, so it seemed necessary to undertake another study in which the entrance to the room was indicated. This conception of the study suggested that additional information could be obtained on the ecology of retreat and active defense by varying the location of walls and aisles and the table size.

## *Method*

The present study involved four diagrams, each one drawn on a separate 8½ × 11-inch sheet.

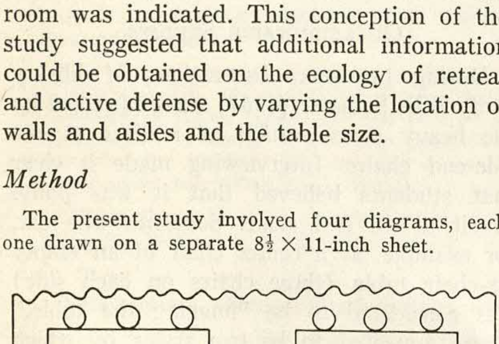

FIG. 1. Arrangement of tables and chairs in Form G.

*Form G* showed eight rectangular six-chair tables, with a large aisle down the center and two smaller aisles along the walls. (See Figure 1.)

*Form H* was the same as Form G, only the tables were set against the wall and the center aisle was wider.

*Form J* was a hybrid of G and H, with the right row of tables against the wall and the left row of tables away from the wall.

*Form I* contained one row of four-chair tables and one row of eight-chair tables, with aisles in the center and along both walls.

Four different sets of instructions were used with the forms (two defense styles and two densities), but any single subject received only one set. One form asked the subject where he would sit if he wanted to be by himself and away from other people—the retreat instructions. The other form asked where he would sit if he wanted to keep other people away from the table—the active defense instructions. In each case, the prospective room density was also indicated. On half the questionnaires, it was stated that room density was likely to be low throughout the day and very few people would be using the room, while remaining subjects were told that room density was likely to be high and many people would be using the room. All the instructions described the room as a study hall such as that already existing in the campus library, and the respondent was informed that he was the first occupant in the room, so he could take any seat he wanted. Booklets containing some combination of instructional set (Defense Style × Room Density) and two diagrams in random order were passed out randomly among 280 students in introductory psychology classes.

## *Results*

*Hypothesis 1* stated that during the retreat condition people gravitate to the end chair closest to the wall. During the active defense condition they make greater use of the center and aisle chairs. Hypothesis 1 was confirmed beyond the .01 level. During the retreat conditions 76% of the subjects occupied a wall chair compared to 48% during the active defense condition.

*Hypothesis 2* stated that with the retreat instructions the subjects face away from the door, while they face towards the door with the active defense instructions. The data disclose a preference in all conditions for a subject to sit with his back to the door—60% of the subjects faced away from the door compared to 40% who faced towards it. However, the results were still in the predicted direction since 44% of the subjects in the active defense condition faced the door compared to 36% in the retreat condition ($p <$ .05).

Although the authors had imagined that the use of different-sized tables and the variation in wall placement would influence seating patterns, specific hypotheses had not been formulated. In all conditions there was a marked preference for chairs towards the rear of the room. Overall, 79% selected chairs in the rear half of the room. However, occupancy of the rear was significantly higher with the retreat instructions under high room density than in any of the other conditions ($p <$ .05). There was also a highly significant preference for the four-chair tables when they were paired with the eight-chair tables, with 73% selecting a small table compared with 27% selecting a large table. There was a slight trend in the active defense condition to make greater use of the small tables, but this was not statistically significant.

When tables against the wall were paired with tables with aisles on both sides, 62% of the subjects selected a table against the wall compared to 38% who chose a table with aisles on both sides ($p <$ .001). As an independent variable, description of the projected room density as high or low made very little difference in where people sat. However, density interacted with the defense instructions on several of the tabulations. With high density *and* retreat instructions, there was significantly greater use of (*a*) the rear half of the room, (*b*) a wall compared to an aisle table, and (*c*) the chair closest to the wall. In essence, the attribution of high room density increased the degree of physical retreat. It had no observable effects on the active defense conditions.

The results make it clear that room dimension and the location of barriers must be considered if we are to understand the ecology of spatial defense. In a library reading room, the best chair for retreat is at the rear, facing away from the door, next to a wall, and at a small table if one is available. Distance from the door protects the person against people simply walking by as well as lazy intruders who are more likely to sit in the first available chair; facing away from the door tends to minimize distraction and also displays an antipathy toward social intercourse; a wall table protects a person's entire left (or right) side; and a small table reduces the number of invaders in close proximity. At this point the authors felt they had derived many useful hypotheses from the questionnaire data which they wanted to extend using an experimental approach under natural conditions. The first experimental studies took place in two soda fountains, and the remainder took place in library areas.

## EXPERIMENTAL STUDIES

Most territories are marked and bounded in some clear way. In the animal kingdom, markers may be auditory (bird song), olfactory (glandular secretions by deer), or visual (bear-claw marks on a tree). Since humans rely almost exclusively on visual markers, the authors decided to test the strength of various markers ranging from the physical presence of a person to impersonal artifacts.

### *Study 1*

The first study took place in a popular soda fountain on campus. The soda fountain was located in a converted office building which still contained a number of small rooms. Patrons would obtain their refreshments at a central counter and then repair to one of the smaller rooms to eat and chat informally. Prior to the study, the authors had been struck by the sight of students walking up and down the corridor looking for an empty room. One of the small rooms which contained three square tables, each surrounded by four chairs, was used for the study. A 20-year-old girl who appeared to be studying stationed herself at a table facing the door. On other occasions during the same hours she stationed herself down the hall so she could observe who entered the experimental room. A session took place only when the room was unoccupied at the outset.

If an all-or-none criterion of room occupancy is applied, the experimenter's defense was not very successful. During only 1 of the 10 experimental sessions was she able to keep the entire room to herself. The average length of time before the room was occupied during the experimental sessions was 5.8 minutes compared to 2.6 minutes during the control sessions, but the difference was not statistically reliable. Although the experimenter was unable to keep the room to herself, she was able to protect the table at which she studied. The remaining three seats were occupied only once during the experimental sessions compared to 13 occupancies during the control sessions ($p < .01$). It seems clear that territorial defense in a public area is not an all-or-none affair. The defender's presence may be seen in a delay in occupancy rather than an absence of invaders and in the avoidance of a subarea within the larger area.

### *Study 2*

The next study took place in a more traditional open-plan soda fountain and, instead of the physical presence of the experimenter, three sorts of objects were used as territorial markers—a sandwich wrapped in cellophane, a sweater draped over a chair, and two paperback books stacked on the table. In each case the experimenter located two adjacent empty tables and arbitrarily placed a marker on one with the other as a control. Seating himself some distance away, he was able to record the duration of time before each table was occupied. The sessions all took place at moderate room density. There were 8 sessions with a sandwich marker, 13 with a sweater, and 20 with the books.

The authors were interested in whether a marker would reserve an entire table as well as the marked chair. The answer for all of the markers was affirmed. The unmarked control tables were occupied significantly sooner than were the marked tables, and the difference was significant for each of the three markers. In fact, in all 41 sessions the control table was occupied sooner or at the same time as the marked table. In only three of the sessions did anyone sit at the marked *chair*. All three were occupied by males, a finding whose significance will be discussed later. It is also interesting to examine the occupancy patterns at the two sorts of tables. The marked tables were eventually occupied by 34 lone individuals and 4 groups of 2 persons, while the unmarked tables were occupied by 18 lone individuals and 20 groups. It can be noted that a group of 2 or 3 could easily be accommodated at a marked table even assuming that the marker represented one person, yet virtually all the groups sat at unmarked tables. It is clear that the markers were able to (*a*) protect the particular chair almost totally, (*b*) delay occupancy of the entire table, and (*c*) divert groups away from the table.

### *Study 3*

A similar study using books and newspapers as markers was undertaken in a dormitory study hall at a time of very light room density. Virtually all the markers proved effective in reserving the marked chair. The only exceptions were two sessions when the school paper which had been used as a marker was treated as litter and pushed aside. After more than 30 individual sessions where virtually all the markers were respected, the authors decided to move the experiments to the main university library where room density was much heavier. It seemed clear that at low densities almost any marker is effective. One qualification is that the object must be perceived as a marker and not as something discarded and unwanted by its former owner. Certain forms of litter such as old newspapers or magazines may, indeed, attract people to a given location.

The locus of study was switched to the periodical room in the university library where room density was high and pressure for seats was great. This room contained rectangular six-chair tables, three chairs to a side. The experimenter arrived at one of the six seats at a designated table at 6:50 P.M., deposited a marker, and then departed to another table at 7:00 P.M. to view any occupancy at the marked position by a student seeking space. During each session, a similarly situated empty chair which was unmarked was used as the control. There were 25 experimental sessions, each lasting 2 hours. The markers included two notebooks and a textbook, four library journals piled in a neat stack, four library journals randomly scattered on the table, a sports jacket draped over the chair, and a sports jacket draped over the chair in addition to the notebooks on the table.

If one compares the average time before occupancy of the marked and the control chairs, it is apparent that all markers were effective. Seventeen of the 25 marked chairs remained vacant the entire 2-hour period, while *all* control chairs were occupied. The average interval before the control chairs were occupied was 20 minutes. Some of the markers were more potent than others. Only one student occupied a chair that was marked either by a sports jacket or a notebook-and-text. Chairs marked by the neatly-piled journals were occupied three of the five sessions, while chairs marked by the randomly placed journals were occupied all five sessions, even though the interval in each case exceeded that of the control chairs. It is clear that the personal markers, such as the sports jacket and notebooks, were able to keep away intruders entirely, while the impersonal library-owned markers (journals) could only delay occupancy of the marked chairs.

An interesting sidelight is that eight of the nine students who sat down despite the markers were males. Since there were more females than males in the control chairs at the same time, the high incidence of males is quite significant. It may be recalled in the previous study that the only three individuals who pushed aside the marker and sat at a marked chair were also males. It is likely that some sort of dominance or risk-taking factor is at work in the decision to disregard a territorial marker. The relationship between personality characteristics and the likelihood of invading someone else's space seems an exciting topic for further investigation.

Another serendipitous finding concerns the role of the neighbor, the person sitting alongside the marked chair, in defending the marked space. In all five trials with the scattered journals, the potential invader questioned the person sitting alongside the marked chair (the neighbor) as to whether the space was vacant. Early in the 2-hour session, the neighbor unknowingly served as the protector of the space. He informed all inquisitive intruders that the space was taken, since he believed the experimenter would return in view of the marker left on the table. As time passed, the neighbor's belief that the experimenter would return to the chair began to wane. At this point he would impart his new conception of the situation to potential invaders, "Yes, somebody was sitting there, but that was over an hour ago. Maybe he's not coming back."

### *Study 4*

Since the role of the neighbor seemed an important aspect of a property-ownership system, the authors decided to investigate it experimentally. The first of such studies involved two experimenters and a person sitting alongside an empty chair. One experimenter seated himself next to a stranger (the neighbor) for 15 mintes and then departed, leaving behind an open book and an open notebook upon the table as territorial markers. After a fixed interval, the second experimenter, in the role of a student looking for a chair, came and inquired about the marked space nonverbally. The nonverbal questionning was a pantomime which included catching the neighbor's eye, pulling out the chair slightly, hesitating, looking at the place markers and at the neighbor, and then back at the markers. The authors had very little experience with such nonverbal cues, but expected that the neighbor's reactions might include verbal defenses ("That seat is taken") and nonverbal defenses (moving the books to reinforce the marker). The independent variable was the length of time between the departure of the first experimenter and the arrival of the second—which was either a 5- or a 20-minute interval. Some sessions had to be terminated when the neighbor departed before the second experimenter arrived on the scene.

Overall the results were discouraging. In only 6 of the 55 trials did the neighbor respond to the nonverbal gestures of the second experimenter in what could be described as a space-defending manner, such as a statement that the seat was taken. Five of the six defensive acts occurred when the experimenter had been away 5 minutes, compared to only one defensive act when he had been away 20 minutes, but considering that there were 55 trials the difference was unimpressive.

### *Study 5*

The authors decided to make another attempt to see if the neighbor could be involved in property defense on a spontaneous basis—that is, if he would defend marked space without being questioned directly. Unlike in the preceding study, the "owner" attempted to establish a relationship with the neighbor prior to the "owner's" departure. There were two phases of the study; when it seemed that the first approach was not leading anywhere, another approach was used. The markers were a neat stack of three paperback books left on the table in front of a chair. The sessions took place at six-chair tables where there was at least 1 empty seat between the marker and the neighbor. The first experimenter entered the room and found the location meeting the experimental requirements (a person sitting at the end chair of a six-person table with two empty chairs alongside him—O-O-S). The experimenter (a girl) sat down on the same side of the table but one seat away (E-O-S). There were 13 trials in each of the following conditions: (*a*) The experimenter sat 5 minutes and then departed from the table, leaving her books neatly stacked on the table. During this time she did not interact with her neighbor. (*b*) Similar to Condition *a*, the experimenter sat for 5 minutes except that during the 5-minute wait, the experimenter asked the neighbor "Excuse me, could you tell me what time it is?" (*c*) Similar to Condition *a*, the experimenter sat for 5 minutes except that during the 5-minute wait the experimenter engaged the neighbor in conversation four times and, while leaving and placing the stack of three paperback books on the table, declared, "See you later." Fifteen minutes later, the second experimenter (a male) entered the room, walked directly to the marked chair, pushed the books directly ahead of him, and sat down at the table.

The results were again discouraging. In none of the 39 trials involving Conditions *a, b,* and *c* did the neighbor inform the intruder that the seat was taken. The authors therefore decided to strengthen the conditions by having the "owner" return and directly confront the intruder. Seven of such trials were added to Condition *a,* six to Condition *b,* and 6 to Condition *c,* making 19 trials in all when the "owner" came back and told the intruder "You are sitting in my chair." Each time she hesitated about 30 seconds to see if the neighbor would intervene, and then she picked up her books and departed. There was no verbal response from the neighbor in any of the 19 sessions. The most that occurred would be a frown or a look of surprise on the part of the neighbor, or some nonverbal communication with someone else at the table. Stated simply, despite a flagrant usurpation of a marked space, all neighbors chose to remain uninvolved. It became clear that if one wanted to study the neighbor's role in such an informal regulatory system one would have to question him directly as to whether the seat was occupied.

### *Study 6*

The next study employed two experimenters, a male and a female, and the same three paperback books as markers. Two different girls were used as experimenters, and the sessions occurred in two different, nearby college libraries. The experimental situation involved six-chair tables where the first experimenter (female) sat down at the same side of a table with a subject, leaving an empty chair between them (E–O–S). The goal of the study was to learn whether a greater amount of interaction between the former occupant and the neighbor would increase the neighbor's likelihood of defending the chair. Unlike in the previous study, the neighbor was questioned directly as to whether the seat was taken. There were three different instructional sets, and these took place according to a prearranged random order. In 14 trials, the first experimenter sat at the chair for 5 minutes without saying anything, deposited the marker (three paperback books), and left. Fourteen other sessions were similar except that at some time during her 5-minute stay, the first experimenter asked the neighbor for the time. Ten other sessions were similar except that the experimenter engaged the neighbor in conversation as to where to get a coke, what was happening on campus, and other minor matters. Fifteen minutes after the first experimenter departed, the second experimenter (a male) entered the room, walked over to the marked chair, and asked the neighbor "Excuse me, is there anyone sitting here?"

The results differ markedly from those in the previous study. A total of 22 out of the 38 neighbors defended the seat when questioned directly on the matter. The typical defense response was "Yes, there is" or "There is a girl who left those books." [2] However, the amount of contact between the first experimenter and the neighbor made little difference in defensive behavior. When there had been no contact, or minimal contact, between the first experimenter and neighbor the seat was protected 58% of the time, while the use of several items of conversation between the experimenter and her neighbor raised the percentage of defensive responses only to 66%. The difference between conditions is small and statistically unreliable; what is impressive is the great increase in defensive behavior when the neighbor was questioned directly. Two other parameters of the situation are (*a*) the time that the first experimenter remained in the seat before depositing her marker, and (*b*) the length of time that the first experimenter was out of room before the second experimenter approached the marked chair.

### *Study 7*

The final study employed two experimenters, both males, and the same three paperback books. The sessions took place at six-chair tables in the library, where the first experimenter again sat down on the same side of the table with a subject, leaving an empty chair between them (E–O–S). He remained either 5 minutes or 20 minutes, depending upon the experimental condition, and then departed, leaving on the table a neat stack of three paperback books. After a designated interval of either 15 or 60 minutes, the second experimenter entered the room and asked the neighbor whether the (marked) chair was taken. The second experimenter recorded the neighbor's reply verbatim just as soon as he was able to sit down somewhere. Since both experimenters were males, it was decided to use only male neighbors in the experiment.

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[2] The neighbors' replies to the intruder's question were scored separately by two coders as indicating defense of the space ("Yes, that seat is taken") or nondefense ("No, it isn't taken" or "I don't know"). There was 100% agreement between the two raters in scoring the replies into defense or nondefense categories.

The independent variables were (*a*) the length of time the first experimenter had been seated before he left his marker and departed and (*b*) the length of time the first experimenter was absent before the neighbor was questioned by the second experimenter. Some sessions were unusable since the neighbor departed before the designated time and could not be interviewed. Most of the unusable sessions occurred when the experimenter had been absent for 60 minutes. The sessions took place at times of light-to-moderate room density.

Although the design had not called for comparison of marked and unmarked chairs, it is noteworthy that the markers were effective in keeping people away. Not one of the 64 marked chairs was ever occupied. Regarding the inclination of the neighbor to defend the marked space when questioned by the second experimenter, a content analysis of the neighbor's responses to the query "Is this seat taken?" into defense and nondefense categories revealed that 44 neighbors defended the marked space by indicating that it was taken, while 20 failed to do so either by pleading ignorance or by stating that the chair was empty. The response to a direct question stands in contrast to the lack of involvement when neighbors were approached nonverbally. The length of time that the first experimenter had originally occupied the chair (his tenure period) had no effect on the willingness of the neighbor to defend the chair. However, the length of time that the previous owner was away—either 15 or 60 minutes—had a significant effect. When the former owner had been absent 15 minutes, 80% of the neighbors defended the space compared to 54% defending it when the former owner had been away a full hour ($p < .05$).

Several aspects of the results require elaboration. It is possible that initial tenure periods of 5 and 20 minutes were not sufficiently different. Yet it seems noteworthy that even with a rather impersonal marker, more than two-thirds of the neighbors defended the marked chair upon direct questioning. Most of those who didn't defend it simply pleaded ignorance ("I don't know if it's taken") rather than indicating that the seat was vacant.

After the experiments had been completed, 15 additional students in the library were interviewed on the question of how personal belongings could reserve space. Each student was asked how he would react if he saw someone intrude into a marked space, particularly if the original owner came back and claimed the space (i.e., the actual experimental situation was described to him). The replies were at variance with what the autors had actually found in such a situation. Most of the respondents maintained that they would indeed protect a marked space, although some of them added qualifications that they would defend the space only if the person were away a short time. Typical responses were: "I would protect the person's books and state (to the intruder) that the place was obviously taken by the presence of the books," and "Yes, I would mention that someone was sitting there." Although the majority mentioned specifically that they would protect a marked chair, in the actual situation no one had done so unless approached directly. The ethic regarding space ownership in the library exists, but is paid lip service, probably because institutional means of enforcement do not exist.

## DISCUSSION

The present article represents a small beginning toward understanding how markers reserve space and receive their legitimacy from people in the area (neighbors) and potential intruders. Psychologists have paid little attention to boundary markers in social interaction, perhaps because such markers were regarded as physical objects relegated to the cultural system (the province of the anthropologist) rather than an interpersonal system which is the true province of the social psychologist. Generally it is the geographers and lawyers who are most concerned with boundaries and markers. Since the present studies took place in public spaces, we are dealing more with norms and customs than with legal statutes. Stated another way, the situations involve an interpersonal system where sanctions are enforced by the individuals immediately present. Goffman (1963) labels the situations the authors used in the experiments *temporary territories*. It is clear that a person placing his coat over the back of a chair desires to reserve the space, and most people in the immediate vicinity will support his claim if questioned (although they will remain uninvolved if they can); such behavior meets Hediger's (1950) definition of territory presented previously as well as the more simple one provided by Noble (1939) that a territory represents "any defended area." The phenomena the present authors have studied do not belong under other available rubrics of spatial behavior, such as home range, biotope, niche, or life space. The major differences between the experimental situations and more enduring territories is that the latter are meshed with a legal-cultural framework and supported in the end by laws, police, and armies. The marked spaces in the present authors' experiments have no legal status and are supported only by the immediate social system. Occasionally it became necessary to articulate the structure of the system by "requiring" neighbors to enter the situation.

People are now spending an increasing portion of their time in public or institutional spaces, including theaters, airport lobbies, buses, schools, and hospitals, where the use of personal belongings to mark out temporary territories is a common phenomenon. The study of territories, temporary as well as enduring ones, deserves study by psychologists. There is some danger that such work will lose much of its force if some semantic clarity is not obtained. While the ethologist's definition of a territory as "any defended area" has considerable heuristic value, there is no need to assume that the mechanisms underlying human and animal behavior are identical. The paucity of data about human territorial behavior makes it most reasonable to assume that the mechanisms are analogous rather than homologous.

In conclusion, the present series of studies suggests that further investigation of spatial markers is feasible and warranted. The physical environment has for too long been considered the background variable in psychological research. The time is past when we can have theories of man that do not take into account his surroundings. Boundary markers not only define what belongs to a person and what belongs to his neighbor, but also who he is and what it means to be a neighbor in a complex society.

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# PERSONAL SPACE AND SELF-PROTECTION[1]

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Personal space is theorized to act in part as a buffer zone which serves as protection against perceived threats to one's emotional well-being. It is predicted that greater spatial distances will be used under stress conditions, by highly anxious people, and by people who perceive their body-image boundaries as weak or unstructured. Such personality variables were assessed by the Rorschach. Personal space was measured by having subjects approach each other, take a seat close or far from the experimenter, and trace one silhouette in relation to another. The stress condition involved having the subjects' physical attractiveness called into question. Results were: a lack of consistency between the three experimental measures of personal space; a highly significant stress effect; and a lack of relationship between personal space and the personality variables.

Recently there has been increasing recognition of the significance of physical space in social interaction. In addition to territoriality, which connotes fixed geographic location, a concept of the significance of distances between individuals has evolved. The concept of "personal space" has arisen to refer to the space immediately surrounding an individual which he feels to be personal, to belong to himself. Depending on various factors, people attempt to maintain certain distances between themselves and others. Research studies recently reviewed by Sommer (1967) have demonstrated the relevance of such variables as culture, ecological setting, the interpersonal relationship, feeling states, and personality.

The present study attempts to extend the research on two of the determinants of personal space: the psychological environment (feeling state) and subjects' personality characteristics. The perception of threatening elements in interpersonal situations, whether originating predominantly from environmental or intrapsychic sources, is seen to call forth measures for self-protection. Personal space may be conceived in the sense of a body-buffer zone (Horowitz, Duff, & Stratton, 1964) that can be used for such protective purposes. This applies to threats to one's self-esteem as well as to the threat of bodily harm. Thus, environmental or internal threats to self-esteem should produce greater spatial distances. Leipold (1963) and Little (1966) have reported some tendency for spatial distances to increase under conditions of stress in the sense of reproof by others. The present research envoked stress as a threat to the self-concept and body image through use of peer ratings of social competence and sexual attractiveness. Leipold's study also found a tendency for males, but not females, who scored high on the Taylor Manifest Anxiety scale to maintain greater distance. In the present study Elizur's (1949) scale of anxiety for the Rorschach was related to personal space. Finally, Horowitz (1966) and Fisher and Cleveland (1958) have presented conceptual formulations which suggest that the Rorschach variable of body-image boundary may be related to personal space. A person with a weak body-image boundary is held to "create exterior conditions which will artificially provide a substitute boundary [Fisher & Cleveland, 1958, p. 355]." Such a perception of one's body boundary as lacking definite structure and substance (the low barrier score) or as weak and permeable (the high penetration score) should result in greater use of personal space.

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[1] This study is based on a thesis submitted by the senior author to Eastern Michigan University in partial fulfillment of the requirements for the Master of Arts degree. Further detailed information is available in the thesis. Thanks are expressed to Francis M. Canter and Carol J. Guardo for comments on a previous draft of the manuscript and to Stephen K. Bedwell, Mary L. Smith, Janice V. Martin, Cynthia E. LaPrad, and Ralph Kirk for assistance during data collection and analysis.

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From the conceptualization of personal space as a means of protection against external threat it was predicted that more personal space would be used under conditions of stress than nonstress. The notion that personality weaknesses may be supplemented by spatial usage led to the predictions that people high in anxiety or penetration or low in barrier would use more personal space.

A further purpose of the present study was to compare various methods that have been employed to study personal space. The use of spatial distance was observed in the placement of miniature silhouette figures (as Little, 1965); in sitting near or far from the experimenter (as Sommer, 1959; Leipold, 1963); and in the distance maintained when approaching another person (as Horowitz et al., 1964). In addition, two exploratory questionnaires were used. The individual consistency over the three experimental techniques and two questionnaires provided an indication of the degree to which the indexes represented a unitary phenomenon.

## METHOD

### *Subjects*

Subjects were introductory psychology students at Eastern Michigan University who participated in the study as part of their course requirements. They were tested in groups of five or six subjects. Each group was scheduled to have six members, three men and three women, but occasionally one person did not come. The final sample consisted of 32 groups totaling 186 subjects, 91 males and 95 females.

### *Personality Measures*

*Body-image boundary and anxiety.* Both body-image boundary measures and anxiety were assessed by the Rorschach. The Harrower group form of the Rorschach was used, with the ink blots presented by means of a slide projector. Following Fisher and Cleveland (1958), subjects were instructed to make three responses to Cards I, II, III, and VIII, and two responses to the others. They were given 1½ minutes to make two responses and 2 minutes for three associations. Following the free association period subjects marked off location and wrote additional percept-related thoughts. The scoring for the barrier and penetration measures followed Fisher and Cleveland (1958): scoring for anxiety followed Elizur (1949). Agreements between two raters (the authors) across 20 protocols were acceptable, with correlations of .81, .89, and .84 for barrier, penetration, and anxiety, respectively.

### *Personal Space Measures*

*Approach.* The first and most direct experimental measure of the use of personal space was a modification of the technique used by Horowitz et al. (1964). Each person in the group approached, one at a time, the other members of the group. The setting was a 12 × 12-foot room which had a one-way mirror on one wall. The two or three males stood, evenly spaced, against one wall and the two or three females stood against the opposite wall, the one-way mirror being perpendicular to both. After the rationales were presented to the stress and nonstress groups (see subsequent explanation), the instructions to all subjects were: "Walk slowly towards the other person; when you reach him or her, stop, and wait until I tell you to return (3 seconds); then return to your position." The subjects first walked to a mark 4 feet directly in front of the object-person and thus made each approach "head on." On the wall over the head of each person was a number from 1 to 6. In random order, constant for all groups, the experimenter called out the 30 approacher–approachee combinations (20 with 5 person groups). On the floor were painted lines 1 inch apart. One or two experimental assistants observed through the one-way mirror from the adjacent room and counted the lines (inches) between the approaching and approached persons during the 3-second pause. (The room was explained to be an optical effects room used by perception psychologists, and only one person in the sample gave any overt indication of suspecting the purpose of the lines.) Scoring the approach measure consisted of obtaining mean scores, in inches, of (*a*) approaches to members of the same sex and (*b*) approaches to members of the opposite sex.

*Silhouette.* The second experimental situation employed a silhouette task in which each subject was instructed to trace a figure representing himself in relation to a printed silhouette of the opposite sex. The distance of the traced silhouette from the printed silhouette, in centimeters, constituted the spatial measure. This method was adapted from that used by Little (1965) and Guardo (1966).

*Seating.* The third experimental measure was similar to the method employed by Sommer (1959) and Leipold (1963). The experimenter was seated at a 4 × 2½-foot rectangular table when the subject walked into the same 12 × 12-foot room. The table was situated in the middle of the room with the experimenter away from, but facing, the door. Two other chairs at the table were an approximately equal distance from the door. One chair was across a corner from the experimenter, the other across the length of the table. As the subject entered the room the experimenter stood and invited him to be seated. Taking the across-the-corner (near) versus the opposite (far) chair was taken as the measure of spatial distance.

*Questionnaires.* The Life Situations questionnaire consisted of a number of multiple-choice questions pertaining to the use of space in the classroom, living quarters, recreational activities, and automobile driving. The Body Contact questionnaire, based on that of Jourard (1966), asked subjects to indicate the extent of "touching" and "being touched by" others since the age of 12. It was considered that greater body contact should correlate with closer spatial distances.

### *Procedure*

Two adjacent rooms were used for the experiment. The written sections were all done in Room A, and the sections involving spatial movement were conducted in Room B. First, the groups were given the Rorschach and the Life Situations questionnaire. Next, the groups moved to Room B, where the approach situation was carried out. Subjects then returned to Room A for the silhouette task and the Body Contact questionnaire. They were instructed that as they finished these questionnaires they were to bring them, one person at a time, into Room B. While subjects were completing the tasks the table and chairs for the seating situation were set up in Room B. Upon entering and being seated, subjects filled out a form on the experiment, were then given brief feedback (the stress-group subjects' attractiveness ratings [see subsequent explanation] were falsified in the positive direction), sworn to secrecy, and excused.

Instructions for the Rorschach were the same for all groups, and differential stress and nonstress instructions did not start until subjects entered Room B for the approach situation. Thereafter, in the stress groups an effort was made to arouse anxiety associated with social competence and sexual attractiveness, while in the nonstress groups the rationales were neutral and geared to keep anxiety minimal.

*Stress.* In the approach situation stress subjects were told:

> This part of the experiment is concerned with the *impression you* make on others when *you* first meet them. A primary aspect of the impression you make is, of course, *your physical* or *sexual attractiveness,* in other words, *your sex appeal.* As you may know, studies have shown that *sexually attractive people* are usually *more successful* in their lives than less attractive people; on the average, they make friends easier, lead more interesting social lives, and are happier and more emotionally stable than less attractive people. Now we are going to see if this is *true* of *each* of *you.* Each of you is going to walk over to every other person. As you do, that person will be *judging you* on *your physical attractiveness* or *sex appeal.* You will also be judged on the impression you make, that is, whether the other person desires to *go out with you* (in the case of the opposite sex) or to be your friend (in the case of the same sex). The judgments will subsequently be compared to each other indicating how you rate when compared with your peers.

The previous statement was read by the experimenter in a serious manner with the italicized words emphasized. The subjects were given small cards on which they rated each other for "sex appeal" and "like as friends or date" on a scale ranging from 1 to 7. For every dyad, the object-person rated the subject just after the approach was made.

On the silhouette task subjects were instructed to imagine a school-building setting with the printed silhouette representing "a classmate whom you would like to date but who has not given any previous indication of being interested." The approach-avoidance conflict was considered to produce tension. For the stress groups the rationale for the "private conference" (seating situation) at the end of the experiment was: "When you come in I will tell you how well you did before, that is, what your sex appeal rating was and the impression you made on the others."

*Nonstress.* In this condition the purpose of the events was made to seem as innocuous and nonthreatening as possible. In the approach situation subjects were told:

> Now I would like your help in studying what is called the orienting reflex. Each of you will be approached by every other person. This is merely to get some idea of certain automatic reflex reactions. These reactions are natural and present in everyone. So relax; there is no need to be nervous. We'll go through this as quickly as possible.

The experimenter read this statement in a relaxed manner with a smile. On the silhouette task the setting was also a school building but the printed silhouette represented "a classmate with whom you are discussing an assignment." For nonstress groups the rationale for a final conference (seating situation) was "to get an idea of your thoughts about the experiment."

## RESULTS AND DISCUSSION

The degree of individual consistency over the three experimental measures of personal space was assessed by intercorrelations, which were obtained separately for each of four groups formed by males and females within the stress and nonstress conditions. The results were: (*a*) Approach and silhouette: A slight positive relationship was found between these measures. The Pearson product-moment correlations were generally positive, but were significant only for the female-stress group. (*b*) Approach and seating: Another slight positive relationship was observed here. All of the point-biserial correlations were positive, but only 1 of 12 was significant at the .05 level. (*c*) Silhouette and seating: The two measures appeared to be slightly negatively related. Of the four

TABLE 1

MEANS AND STANDARD DEVIATIONS FOR FOUR SEX-STRESS GROUPS IN APPROACH DISTANCE (INCHES) AND SILHOUETTE DISTANCE (CENTIMETERS)

| Variable | Male-Stress[a] | | Male-Nonstress[b] | | Female-Stress[c] | | Female-Nonstress[d] | |
|---|---|---|---|---|---|---|---|---|
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Approach | | | | | | | | |
| Same sex | 15.0 | 4.2 | 12.2 | 5.2 | 13.9 | 4.4 | 11.3 | 3.4 |
| Opposite sex | 14.8 | 4.5 | 12.1 | 4.1 | 16.2 | 4.8 | 12.3 | 4.4 |
| Silhouette | 45.4 | 36.2 | 21.7 | 8.8 | 56.3 | 48.6 | 21.2 | 8.9 |

[a] $n = 45$.
[b] $n = 46$.
[c] $n = 47$.
[d] $n = 48$.

point-biserial correlations three were in the direction opposite to consistency, and one of these was statistically significant. (*d*) Questionnaires: The two questionnaires failed to correlate significantly with each other or with the experimental measures. It was concluded that there was little consistency in the use of the three experimental spatial measures and that the results caution against discussion of personal space without consideration of the method of assessment.

The prediction that stress would increase spatial usage was supported for two of the three experimental situations. For the approach situation, an analysis of variance was employed with the between variables of stress versus nonstress and sex of the subject (approaching person) and the within variable of approach toward members of the same versus opposite sex (sex-of-object). The mean scores of the analysis are presented in Table 1, and the results are presented in Table 2. The primary finding, consistent with the prediction, was a highly significant main effect ($F = 24.47$, $p < .001$), and Table 1 indicates that the groups in the stress condition stayed from $2\frac{1}{2}$ to $3\frac{3}{4}$ inches further away than the nonstress groups. There was also a significant interaction between sex-of-subject and sex-of-object ($F = 7.76$, $p < .01$). As Table 1 indicates, this was because females approached closer to the same sex and stayed farther away from the opposite sex, while males used virtually the same distances in approaching the same versus opposite sex. The finding in Table 2 that there was a significant difference in approaching same versus opposite sex obviously reflects the discrepancy in the approach of female subjects. The finding may be readily accounted for by a cultural norm for females to be reserved when interacting with male strangers.

TABLE 2

ANALYSIS OF VARIANCE FOR STRESS CONDITIONS, SEX-OF-SUBJECT, AND SEX-OF-OBJECT ON APPROACH DISTANCE

| Source of variation | *MS* | *df* | *F* |
|---|---|---|---|
| Between variables: | | | |
| Stesss (A) | 78619.05 | 1 | 24.47*** |
| Sex-of-*S* (B) | 200.70 | 1 | .06 |
| A × B | 617.40 | 1 | .19 |
| Error | 2313.19 | 176 | |
| Within variable: | | | |
| Sex-of-object (C) | 5461.20 | 1 | 6.69* |
| A × C | 387.45 | 1 | .47 |
| B × C | 6332.40 | 1 | 7.76** |
| A × B × C | 1034.55 | 1 | 1.27 |
| Error | 815.60 | 176 | |

* $p < .02$.
** $p < .01$.
*** $p < .001$.

The group nature of the approach situation permitted subjects in a group to witness one another's performance. This suggests that modeling or group norms may have been operative and that there would be differences among the 16 experimental groups in each condition. One-way analyses of variance compared group means and yielded significant differences for both the stress groups ($F = 2.09$, $df = 15/76$, $p < .05$) and nonstress groups ($F = 6.09$, $df = 15/80$, $p < .01$). Since a modeling effect would tend to magnify the stress-nonstress difference in individual data, the approach measure was also analyzed using the group as the unit of sampling. A *t* test between the group means of the stress and nonstress groups yielded a significant difference in the predicted direction ($t = 3.44$, $df = 30$, $p < .01$). In sum, the results using group means support the hypothesis of increased spatial distance under stress, but caution against use of the approach measure without consideration of modeling effects.

In the silhouette situation chi-square analyses were employed to assess the effects of the stress condition. The results showed the stress effect to be highly significant for males ($\chi^2 = 22.16$, $df = 1$, $p < .001$) and for females ($\chi^2 = 11.43$, $p < .001$). Inspection of mean scores presented in Table 1 shows that, consistent with expectation, the distance in the stress condition was greater than that in the nonstress by 23.7 centimeters for males and by 35.1 centimeters for females.

For the seating situation chi-square tests of stress versus nonstress conditions were not significant for males ($\chi^2 = .04$) or females ($\chi^2 = .05$). The results may have stemmed from the (inadvertent) arousal of stress in all subjects due to the Body Contact questionnaire, which was presented immediately prior to the seating situation. Also relevant is the seating task itself, which, as mentioned, correlated weakly with the other two experimental measures. A relationship between stress and seating distance may have been masked by the dichotomous nature of the response alternatives or by other covarying factors, such as the opportunity for eye contact. In this area it might be fruitful to have subjects place portable chairs in relation to that of the object person and to develop a method for systematizing and charting the resulting arrangements.

The personality variables of anxiety and body-image boundary were correlated with the three experimental measures of spatial distance for each of the four sex-stress groups. For each personality variable there were 16 correlations with the spatial measures: 8 with approach (4 for same sex approaches, 4 for opposite sex approaches), 4 with silhouette, and 4 with seating. In all instances statistical significance was determined by two-tailed tests. Only 2 of 48 personality-variable correlations were significant (one each for the anxiety and barrier scales), and only 20 of 48 were in the predicted direction—results which are no higher than chance. The results for the two questionnaires were similarly at a chance level. The predictions of relationships between personal space and the personality variables were thus not supported.

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# BODY MOVEMENT AND SPEECH RHYTHM IN SOCIAL CONVERSATION[1]

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The relationship between body movement and speech rhythm was newly formulated following Boomer's work on hesitations in speech: movements were predicted to occur early in phonemic clauses and at points following nonfluencies within clauses. A preliminary study of old data for which the movements were located by watching motion pictures bore out the prediction, and led to a more intensive study using more representative speech samples, and recording techniques designed to eliminate possible artifacts. The results were highly significant, but the amount of movement variance accounted for was small. The data collected by this method allowed direct test of statements by Pittenger, Hockett, and Danehy, and by Scheflen, whose claims of very close speech-movement relationships were found to be exaggerated. The linkage found between hesitations and movements was interpreted in terms of speech encoding processes.

The research to be reported here was occasioned by overlapping findings which called a method into question. The original research was on the relationship between body movements and moods or feeling states in interviews (Dittmann, 1962). Its purpose was to learn whether frequency of movements of head, hands, and feet could serve as cues to changes in psychological states, as Sainsbury (1955) had found with arm movements alone. The results from intensive study of a single patient were encouraging: what Sainsbury had shown with electromyographic techniques could apparently be duplicated by counting movements from motion pictures of interviews, and adding more body areas to the analysis made visible the differential patterning of movements across moods, reminiscent of results from autonomic research.

The interview passages used in this study were subsequently examined by Boomer (1963) from the standpoint of certain speech variables. His results questioned the usefulness of counting body movements, since movement output and speech output were found to be quite closely correlated: number of words per passage and number of movements correlated .61. The implication of Boomer's findings for the movement-mood study is obvious. The results could have been artifacts of the different amounts of speech in the interview passages which represented the different moods in the study. Statistical control through analysis of covariance was used to provide an immediate test of such an artifact, and the original conclusions were upheld. In addition, an experiment was performed using normal control subjects to learn if movements would covary with speech output when emotional disturbance was held constant (Boomer & Dittmann, 1964). In this experiment speech output was varied by instructions to the subjects, but movement frequencies as counted from motion pictures of the sessions were not affected. So the correlation between speech and movements which Boomer had found seemed to depend upon covariation in the third factor of mood.

There is, however, another meaning of the speech-movement relationship, one closer to common sense impressions, which the work so far had not touched. Ordinarily we think of movements during speech as concomitant gesticulations. It is in this sense that Renneker (1963) described what he called speech-accompanying gestures, which "seek to complement, modify, and dramatize the meanings of words [p. 155]" In the same sense Ekman (1965) wrote of an accenting function of nonverbal behavior, and Freedman and Hoffman (1967) separated what they called punctuating movements from other speech-related movements. In our work we had obscured the speech-accompanying aspect of movements by totaling both words and movements over a relatively long unit, 1½ minutes in the mood-movement study and 3 minutes in the controlled experiment. A unit which had more to do with the nature of speech was clearly needed.

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[1] This paper is an extension of one given at a symposium chaired by Paul Ekman, titled "New Approaches to the Study of Facial Expression and Body Movement," at the American Psychological Association meetings in New York, 1966.

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Such a unit is the phonemic clause of Trager and Smith (1951), which has been the basis for a number of psycholinguistic studies in our laboratory (Boomer, 1965; Boomer & Dittmann, 1962; Boomer & Laver, 1968; Dittmann & Llewellyn, 1967, 1968). It consists of a string of words, averaging five in length, in which there is one and only one primary stress and which is terminated by a juncture, a barely perceptible slowing of speech, often with slight intonation changes at the very end. This unit does take account of the natural rhythm of speech, and offered promise for further speech-movement investigations. One of the studies (Boomer, 1965) even provided a model for this work. Locating hesitation forms (filled and unfilled pauses) in relation to phonemic clauses, Boomer found that these forms are not distributed randomly in the flow of speech, but tend to bunch up toward the beginnings of clauses. The parallel studies of movements reported here were designed to settle the methodological questions so that work on body movements and emotional states could proceed. In addition to this, the results have implications for a growing number of other reports on the relationship between verbal and nonverbal communication. Finally, the results form a clear tie with some theorizing from our laboratory on encoding and decoding speech.

The data for the first exploration came from the experiment on speech and movement, the same data used in the hesitation study. The material had been segmented into phonemic clauses for that study, and movements of the three body areas had been entered onto the typescripts at the words where they occurred by watching the motion pictures, a technique about which we shall have more to say. Purposive movements, like adjusting eyeglasses, and movements which were substitutes for words were not included in the counts. Examination of the typescripts thus marked led to the impression that movements occurred both at the beginnings of clauses, and also at nonfluencies within clauses and at the start of speech following those nonfluencies. The relationship between speech rhythm and body movement was then formulated for test in this way: Movements will be found at what we called *start positions* more often than expected and at *nonstart positions* less often than expected. In the following passage, the subject is talking about his hobby, rebuilding old automobiles, and in particular about his Model A Ford:

```
it's ah [pause] green with yellow wheels
                            foot

[pause] and ah [pause] proves to be quite a sensation
            hand        head                hand

around the neighborhood
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Each line in this passage consists of one phonemic clause, and pauses are nonphonations of 200 milliseconds or more as determined from oscillograph records of the speech. The first clause contains three start positions: the beginning word, the pause (in this case a filled pause), and the first word, "green," after the pause. It also contains three nonstart positions, the fluent final words of the clause. The second clause contains four start positions: the initial pause, "and," the filled pause, and "proves"; the nonstart positions are the last four words. The third clause contains one start and two nonstart positions. Movements are tabulated by their locations at these two types of positions within clauses.

The results of our exploratory study confirmed our initial impression: movements occurred at start positions more often and at nonstarts less often than the relative frequencies of these positions would lead us to expect. Even though the relationship was statistically significant, however, it was not a close one, as we shall explain more fully below.

There were three shortcomings to the exploratory study which needed clearing up before we went on. First, the speech samples were not very representative of interviews or social conversations: the base-line condition in the experiment from which they were drawn was not a conversation at all, but rather a short, extemporaneous speech. The other two shortcomings were potentially more serious, since both could have meant that we were making a spuriously high estimate of the speech-rhythm–body-movement relationship from our results.

1. Locating movements in the stream of speech as we had done from the motion pictures of the sessions is beset by a phenomenon discovered by Ladefoged, who found that

> although he is a trained phonetician accustomed to listening to speech analytically, he was unable to note exactly when a switching click occurred in a recorded utterance which was being played over again in a loop of tape [Ladefoged & Broadbent, 1960, p. 1962].

This observation led to a series of experiments using a variety of materials and a number of controls. In all cases the perceived sound was found to come earlier than it actually did, and the error was greater with connected language than with other material. Looking at raw data which they present on sentences in the light of our results, we find that subjects tended to displace the sounds toward the beginnings of what were probably phonemic clauses as they were read. Thus we could have made systematic errors in locating movements which would then have given rise to the impression that movements occurred early in phonemic clauses. Our preliminary results might have been simply a confirmation of the Ladefoged effect. This possibility is compounded by the fact that identifying movements in this larger sample turned out to be a less reliable procedure than we had originally been led to believe by our first patient, who produced discrete and easy-to-tabulate movements.

2. In the definition of start position we included nonfluencies, which are most frequently pauses. The proportion of time in conversational speech taken up by pauses varies from situation to situation, but is usually somewhat longer than that taken up by speech. More time was therefore available at start positions for movements to occur, thus possibly biasing the results in favor of more movements appearing at starts.

To overcome these shortcomings we collected new data on longer interviews of a more conversational nature, using recording techniques which enabled us to circumvent or examine the effects of the possible artifacts mentioned above. In addition, part of the new data permitted direct comparison with Boomer's findings that hesitations are most often found at the beginnings of phonemic clauses.

## METHOD

### *Subjects*

The subjects were 12 college student volunteers, 6 males and 6 females, who were serving as normal controls during a 3-month stay at the Clinical Center of the National Institutes of Health. The subjects were told at the time they were asked to participate that the research program concerned various factors in conversational speech, and that we were recording speech and body movements in this particular study.

### *Procedure*

Each subject was interviewed (by the first author) for a period of approximately 15 minutes on each of 2 successive days. The interviews were informal and the subject was allowed to talk about anything that interested him. Upon entering the experimental room the subject was seated comfortably in a chair, and the experimenter attached the movement transducers, giving a detailed explanation of their operation. The subject was assured that though the transducers might seem obtrusive at first, people got used to them fairly quickly. The experimenter then took a nearby seat, asked the subject to begin talking, and kept the conversation running at a leisurely pace, trying to maintain a minimum of stress.

### *Apparatus*

The movement transducers were small accelerometers consisting of replacement crystal phonograph cartridges such as the Electro-Voice 51 or 83, or the Astatic 81T or 90, with a drop of solder cemented to the needle. This assembly was encased in a thin-walled brass tube $\frac{7}{8}$ inch long and $\frac{1}{4}$ inch in diameter, altogether weighing about 3 grams. Lead wire was miniature coaxial cable, RG174/U. Four of these transducers were sewn to strips of Velcro self-adhering tape, and attached to both hands and both shoes. Another was secured to a semicircular plastic band used by women to hold hair in place and slipped over the subject's head. The outputs from the two hands were paralleled, as were those from the two shoes. A lavalier microphone picked up the subject's speech.

In the adjoining room the voice and movements were recorded. The signals from each body area were shaped to provide $\frac{1}{2}$-second pulses to drive three event markers on an Esterline Angus operations recorder running at .75 inch per second. No differentiation was made, of course, between different types of movements, such as the purposive movements which were eliminated in the exploratory study. Speech was recorded on both a tape recorder and an oscillograph mounted on the operations recorder. A timer switched a brief 1-kiloHertz signal into the audio tape every 15 seconds and also moved a fourth event marker on the operations recorder so that the two records could later be collated precisely.

The first typescript from the tape recording was segmented into phonemic clauses by the method described in our 1967 report. The typescripts were then collated with the Esterline Angus strip chart so that the pauses over 200 milliseconds could be located. The typescripts were then retyped, one phonemic clause per line, as illustrated above. The final step was to transfer the head, hand, and foot movements from the strip chart to the revised typescript with as much precision as possible. We felt that despite occasional difficulty in locating individual words on the strip chart when the subject talked too fast or too softly, this method represented a substantial improvement in accuracy and objectivity over that of working from films of sessions.

Tabulations were made of movements at start positions and at nonstart positions for each body area for each subject. Expected frequencies for movements at these two positions were derived from the proportions of the total start and nonstart positions, and the resulting frequencies cast into a 2 × 3 × 2 × 12 partition of the Chi-Square (see Sutcliffe, 1957): Clause Location (start versus nonstart positions); Body Area (head, hands, feet); Trials (first versus second interview); and Subjects. Since independently derived expected frequencies were available only for Clause Location, only those partitions which included this dimension could be tested.

## RESULTS

The percentages for the Clause Location and Body Area dimensions, both obtained and expected, are given in Table 1. The results strongly supported those of our exploratory study. Clause Location, the only testable main effect, was highly significant ($\chi^2 = 1{,}221.23$, $df = 1$); movements were more likely to occur at start positions, and less likely at nonstarts, than would be expected by chance. This relationship held consistently for all individual subjects and for all body areas within subjects. The interaction between Clause Location and Subjects was also significant ($\chi^2 = 167.21$, $df = 11$), indicating that the strength of the relationship is dependent upon subject variability. Indeed the ratio of observed to expected movements at starts differed widely from individual to individual. For one subject it was almost 2:1; for the average it was about 3:2; while for three subjects the ratio was less than 4:3.

TABLE 1

PERCENTAGES OF MOVEMENTS FROM THREE BODY AREAS AS RELATED TO SPEECH RHYTHM

| Body area | Start | | Nonstart | | Total |
|---|---|---|---|---|---|
| | Obtained | Expected | Obtained | Expected | |
| Head | 13 | 10 | 11 | 14 | 24 |
| Hands | 32 | 21 | 20 | 31 | 52 |
| Feet | 14 | 10 | 10 | 14 | 24 |
| Total | 59 | 41 | 41 | 59 | 100 |

Note.—Number of movements on which these percentages are based: 8,727.

The Body Area × Clause Location interaction was significant but comparatively small ($\chi^2 = 29.10$, $df = 2$); while all three body areas produce movements related to speech rhythm, the hands and feet conform more closely than does the head. We also found a significant triple interaction between Clause Location, Body Area, and Subjects, and a significant interaction of all four variables, but the chi-squares were relatively small for this large $N$ and inspection of the data did not reveal any marked and easily interpretable patterns.

This analysis serves to tell us that there is a highly significant relationship between speech rhythm and body movement, but it does not give us an estimate of the strength of the relationship. For this purpose the original data were converted to rates and submitted to analysis of variance. Movements at starts per 1,000 start positions, and movements at nonstarts per 1,000 nonstart positions, were computed separately for each subject and for each body area within subjects. The distributions were markedly skewed, as is common with rates, and contained several entries which were numerically small. These problems could be corrected quite adequately by a square-root transformation of the form $X'_i = \sqrt{X_i} + \sqrt{X_i + 1}$.

The variables of clause location and body area as repeated measurements within subjects were analyzed. The main effects of Clause Location and Body Area were significant and the interaction between the two, but nothing else. In spite of these "significant" effects, however, the amount of movement accounted for was small. When $\omega^2$s were computed from the results (see Hays, 1963, pp. 406–407), Clause Location was found to account for 7% of the total variance and Body Area, 5%. The percentage of variance accounted for by Clause Location is very similar to the 7.8% found in the exploratory study, for which a similar analysis was carried out. We shall have more to say about this similarity later on in the Discussion section. Of the remaining variance, between subjects and error, the former was larger by a sizable amount. Indeed the range of total movement output from subject to subject was astonishing. Recall that subjects were allotted a total of 30 minutes of speech during the two interviews. One subject produced only 40 movements of all body areas during that time, while another moved 1,934 times! Clearly individual differences are a most important contributor to any counts of body movements in conversation.

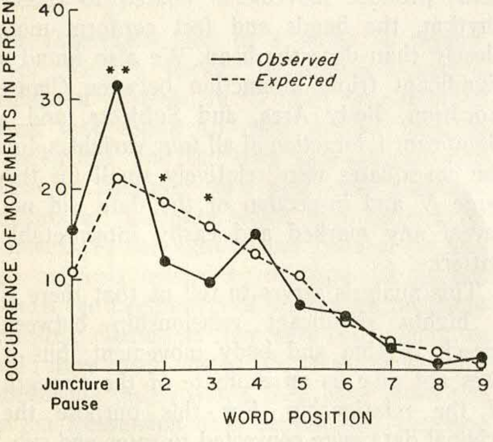

Fig. 1. Percentages of movements in fluent phonemic clauses at successive word positions, compared with percentages expected by chance. ($*p < .05$; $**p < .01$.)

Now we turn to the control for the final shortcoming of the exploratory study mentioned above, namely that including pauses in the definition of start position means more time available for movements at those times. A by-product of this control was that we could compare location of movements within clauses with Boomer's findings on hesitations. We used material from another set of interviews, one collected to study trends in movement patterns in 12 sessions covering a 2-month period. Two subjects were used for that study, and the procedure was the same, except that the transducer for head movements was the more sensitive one developed for the study we reported in 1968. Five interviews from each subject were studied, the first, second, sixth, ninth, and twelfth, selected to represent the entire range of time. For the present purposes a random sample of 20 phonemic clauses was drawn from each of the five interviews from each subject. Only the fluent clauses lent themselves to graphical representation directly comparable to Boomer's Figure 1 (1965, p. 152). These comprised 117 of the total sample of 200 clauses. A fluent clause is defined as one which does not contain any hesitation pauses or retraces, but may be preceded by a juncture pause, since such pauses are external to the clause itself. The 117 clauses varied in length from 1 to 9 words, and some were preceded by juncture pauses. Tabulations were made of the word positions of all of these clauses, and of the movements at each position. Multiple movements at a position, an infrequent occurrence, were counted as one. The frequencies were then converted to percentages for easier comparison in graphical form, and are presented in Figure 1. The percentages of the different possible word positions in the clauses give us an indication of the distribution which movements should follow if only chance were operating. Significance tests were run on the difference between the movement and chance proportions at each position, using the arcsine transformation (Walker & Lev, 1953, pp.

423–424). The difference at the first word positions was significant (.01), and so were those at the second and third positions (.05). Thus it is the first word of the clause which accounts for most body activity at starts.

The remaining 83 clauses of the sample, which were nonfluent by our definition, were analyzed in an analogous manner, but their results are somewhat more complicated to present. In the majority of these clauses only one word precedes the nonfluency, as Boomer found with a much larger sample of clauses. A few clauses had two words before the nonfluency, and so on down to one clause in our sample which had six antecedent words. Word positions following the nonfluency could be treated as they were in the fluent clauses. Fifteen clauses had two or more nonfluencies. The frequencies were too small to be useful, and were added to the comparable positions following the first nonfluency. Figure 2 shows the results. In only one of the word positions of the nonfluent clauses did the number of movements differ significantly from chance, and this was the first word following the nonfluency. Combining what we have found in our examination of both types of clause, we may say confidently that the major contributor to the high frequency of movements at starts is the beginning of speech, not the pause.

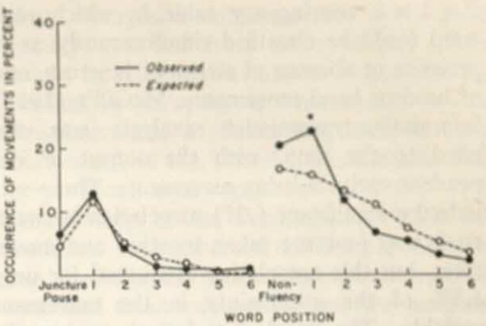

Fig. 2. Percentages of movements in nonfluent phoneme clauses at successive word positions, compared with percentages expected by chance. (*$p < .05$.)

The terms of our analysis are different from Boomer's, since in his study of the location of hesitations the "position" is necessarily the word boundary, whereas in our work it is the word itself. Nevertheless, we can say that like hesitation forms, movements appear early in phonemic clauses where the clause is run off fluently by the speaker, and that movements follow hesitations closely where the clause is nonfluent. Our results thus point strongly to a close association between movements and hesitation forms in speech.

### Relationships to Other Research

Some of our results and methods are obviously relevant to the burgeoning recent literature on the relationship between verbal and nonverbal means of communication. In the case of two published statements our methods permit direct comparison of results. These concern the way movements seem to coincide with stress and juncture in the phonemic clause, and the authors use these terms in the same sense that we have been using them. The first statement, by Pittenger, Hockett, and Danehy (1960), ties movements to stress patterns in phonemic clauses. In connection with the notion that a message in one channel (such as body movement) may serve to emphasize or reinforce a message in another (such as speech), they wrote, "Most speakers of American English accompany their primary stresses with slight jerks of head or hand [p. 240]." The other statement is from an article by Scheflen (1964), referring to unpublished work by a colleague. "In 1962 Birdwhistell demonstrated that these junctures [which terminate phonemic clauses] were also accompanied by a movement of the head, eyes, or hands." Illustrations are given of the "occurrence of this pitch and kinesic activity at the termination of each syntactic sentence . . . [p. 320]."

Juncture and stress are closely related phenomena: the primary stressed word in most phonemic clauses occurs just before the juncture. These authors are thus saying that head movements are regularly found at the end or close to the end of phonemic clauses. Our results show that more movements are to be found earlier on in the clause, but as we have pointed out, this relationship by no means accounts for all of the movements. By our methods we can also study the association between head and hand movements on the one hand and stress and juncture on the other. Using the same sample of 200 phonemic clauses referred to just above, we set up a

2 × 2 × 2 contingency table in which each word could be classified simultaneously as to presence or absence of stress, of juncture, and of head or hand movements. McGill's (1954) information-transmission analysis was applied to the data, with the output or dependent variable being movements. There was indeed a significant (.01) association between stress and juncture taken together and movement, but this association accounted for only 1.3% of the uncertainty in the movement variable. The total association among the variables in the table, however, was very large, and most of this was due to the high correlation between the independent variables, stress and juncture: 79% of the primary stressed words immediately preceded junctures. This correlation meant that the interaction term in the analysis was negative (see McGill, p. 108), and when this term was taken into account to examine the association between movement and either stress or juncture with the other held constant, the results no longer reached traditional significance levels. These last manipulations, however, yielded relatively minor changes and were not consistent from one subject to the other. What was consistent was the large association between stress and juncture, and the very small association between either of these and movement. The raw data make this last point clear: 29% of the primary stressed words in the sample were accompanied by head or hand movements, as compared with the 21% which would be expected by chance. The parallel figures for juncture words were 31% and 22%. These differences are statistically significant, but do not allow such sweeping conclusions as both Pittenger et al. and Scheflen have made.

Our results are also very closely related to Kendon's (1967) extensive study of the function of gaze direction in social interaction. We cannot compare data directly with his because of the difference in dependent variables, but Kendon's analysis of changes in gaze direction at what he calls hesitation pauses and phrase boundary pauses provides an opportunity for exploring convergences be-between the two sets of findings. We can do this because changes in gaze direction frequently accompany head movements, as indicated in an example he presents where both variables are coded for one pair of subjects (p. 32).

In the case of hesitation pauses, Kendon found that the speaker, after looking away from his conversational partner just before and during the pause, looks up again as he resumes speaking. The most abrupt change is at the onset of speech. We should therefore expect to find more frequent head movements at the resumption of speech following hesitation pauses, and indeed that is the way things look in our Figure 2. In the case of phrase boundary pauses Kendon found more gazes at the other person at the end of the last phrase than at the beginning of the next one, with the most abrupt change in direction being during the pause itself. Head movement and gaze direction apparently operate differently in this case, since we find movements more frequently at the beginnings of phrases. We should note that the "phrase" which Kendon used is closer to the grammatical phrase[4] than is the phonemic clause, but the difference is probably not great enough to account for the difference in results.

## DISCUSSION

The really interesting questions which this research has raised are those of why body movements should be located as they are in the rhythmic stream of speech, and why they should coincide in their location with hesitation forms in the speech itself. Our explanation of the movement–speech-rhythm relationship is mainly concerned with the process of speech encoding, and is a logical extension of previous work in our laboratory (Boomer, 1965; Dittmann & Llewellyn, 1967). Forming one's thoughts and ideas into words is an astonishing accomplishment, and the complications of performing the task are shown by the many hesitations and slips in speech. All of these, we submit, are outward signs of encoding difficulties. Among the reasons behind these difficulties are the complexity of the idea to be expressed; the impression the speaker is trying to make on the hearer; wishes or fears which the speaker might have about the subject-matter he is talking about, or about associated topics; pressures for production in the immediate social situation surrounding the speech event; difficulties with the syntactic relationships in and of themselves. These and probably many other factors impinge on the speaker repeatedly to interfere with smoothness in speech.

It is our contention that whenever factors like this intervene, tension builds up within the speaker. The most direct reflection of this tension is the hesitation, and next are the other forms of nonfluency. These are the most "natural" and therefore the most likely expressions of encoding tension. In some cases the tension may spill over into the motor sphere, producing movements of the type we have been studying here. The mechanism for this is not clear, but the concept of cognitive tension spreading to motor responses is not a new one (for a review of related literature, see Duffy, 1962). How often some actual motor behavior results from this process varies widely. Both group and individual styles play a part in this variation, and shifts may accompany psychological states in any individual. In any event, the phenomenon of motor spillover is a familiar one to all people, according to the reasoning being presented here. We believe, in fact, that it has come to be used by all people for communicative purposes. Thus if a person wishes to convey the idea that what he is expressing is important or difficult to conceptualize or exciting, he will introduce movements along with his speech to get this extra information across. The timing of these movements will tend to follow the pattern of timing he is familiar with, that is, early in encoding units or following hesitations in speech.

This way of putting the process is at variance with the differences in types of speech-related movements which Freedman and Hoffman have found. From their work we should expect movements having one function in speech to be more closely related to speech rhythm than movements serving some other function. We have no direct evidence to the contrary, and, to be sure, we have only accounted for a small proportion of movement variance by all the variables we have had under control. We can, however, derive some indirect evidence by comparing methods and results of our exploratory study and the main study presented here. Recall in the former that "purposive movements" were deliberately excluded from the protocols. These would certainly correspond to the Literal-Reproductive category of movement in the Freedman and Hoffman list, and to many movements in the Literal-Concretization category. These make up about 10% and 38% respectively, of the movements in their twc patients, each observed in sharply contrasting psychological states. Our exploratory stud excluded these movements, but the main on necessarily included them, since the accelerometers could not differentiate among any type of movements. Still, when the analyses o variance in the two studies are compared, th main effect of Clause Location accounted fo 7.8% of the movement variance in one an 7.0% in the other. Thus whether the movement is "purposive" or not apparently doe not change its location with respect to th speech rhythm. We should stress again tha our evidence for this position is indirect. W would need to apply all of the measures t the same set of data to know for sure.

If indeed movements are most likely to b placed at start positions by our definitior regardless of their function in the social situation, then a number of apparent contradictions between our findings and those of othe investigators disappear. Freedman and Hoffman, for example, said of their category c punctuating movements that "they seem t accentuate what is being said . . . by highlighting the vocal intonation [pp. 531–532]. Similarly, Ekman (1965) wrote of movement "used rhythmically to *accent* or underlin certain words [p. 441, original italics]." I both of these quotations it sounds as if th important words of clauses, the ones mos likely to have the primary stress and to coincide with the terminal juncture in our term were accompanied by a movement, as Pittenger et al. (1960) said they are and a Scheflen (1964) cited Birdwhistell as havin "demonstrated." According to the conceptualization being developed here, this is precisely the way the listener in the conversatio views it; the speaker appears to be emphasizing certain words, and his movements appea to coincide with the words themselves. W have found that a significant, though ver small proportion, of primary-stressed word are indeed accompanied by head or hand movements. The point is that to the naked eye a very large proportion appears to do so, as all the writers referred to above say they do. With two data-collecting techniques, locating movements from motion pictures and from accelerometer outputs, we have shown that this appearance is belied by the facts. Ekman and Freedman and Hoffman could not be expected to corroborate our results, for they were using neither the conceptual tools nor the techniques needed to learn about the relationships we have found. Pittenger et al., on the other hand, had the concepts and Scheflen and Birdwhistell had both the concepts and one of the techniques we used, film analysis, available to them. How these latter came to their stated conclusions is not clear from their published articles, since they have not reported either the methodological details or the data of the studies on which these conclusions were based.

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# GREGARIOUSNESS IN RATS AS A FUNCTION OF FAMILIARITY OF ENVIRONMENT[1]

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Rats were either individually preexposed to an open field or not and then observed in pairs. Preexposed rats were much more gregarious on their first test in pairs than nonpreexposed pairs, but the latter group became more gregarious with repeated testing. Prolonged absence from the open field led to decreased affiliation. In a second experiment, rat pairs were observed for four days in either a black or a white open field and then switched to the other field. Gregariousness increased in either open field, but decreased on switching. Both experiments show that rats are most affiliative in familiar environments, and suggest that fear or curiousity about novel environments may compete with social attraction.

Over the past few years, a number of studies have explored the determinants of sociability in rats (Cappell & Latané, 1969; Joy, 1967; Latané, 1968; Latané & Glass, 1968; Schneider, 1967). These experiments have dealt with a variety of issues ranging from the initial question of whether rats are gregarious to investigations of the stimulus conditions eliciting social approach, the effects of social isolation, and the effects of autonomic arousal on affiliation. Each of these studies, as a by-product of the desire to obtain as much information as possible from each pair of animals, has exposed animals to repeated testing. In every experiment, rat pairs were observed together over a period of days, and measures were taken of their sociability.

Perhaps the most consistent result to emerge from this series of studies is that rats become more gregarious with repeated testing. In the experiments cited above, 141 of 158 pairs of rats (90%) stayed closer together on the last day of testing than on the first. This day-of-testing effect has been statistically significant in each of these studies. It occurs with old rats, young rats, female rats, male rats, rats housed alone, rats housed in pairs, and rats injected with adrenalin, alcohol, caffeine, or chlorpromazine. How can it be explained?

Three general classes of hypotheses are possible. It may be that the effects depend upon continued social interaction in the open field. For example, with repeated testing, rats might learn that social interaction is positively reinforcing, and/or initial feelings of fear or aversion for each other might extinguish through familiarization. Second, it may be that the effects depend only on the rat's experience with the open field itself. If unfamiliarity with the field leads to exploratory tendencies which compete with affiliation, these tendencies should decrease after experience with the field, allowing increasing gregariousness. Finally, it may be that experiences outside the testing situation are entirely responsible for the increase in affiliation over trials.

It was the purpose of the experiments reported here to explore these possibilities and, specifically, to test the hypothesis that increasing affiliation over days is due to increasing familiarity with the open field and not increasing familiarity with the other rats. In the first experiment, some rats had the chance to become familiar with the open field alone before being tested in pairs; other rats were tested in pairs in an unfamiliar open field. In the second experiment, rats were tested in pairs in one open field until it became familiar, and were then shifted to a second, unfamiliar open field. In both experiments, it was predicted that rats would be most gregarious when tested in a familiar environment.

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## EXPERIMENT I

### *Method*

To investigate the effects of familiarity with the test environment on gregariousness, albino rats were either given or not given prior experience alone in the open field before being tested in pairs over a 7-day period. Additional trials were run at 1-, 2-, 4-, or 8-week intervals to determine whether the effects of familiarity were reversible with absence of exposure. Gregariousness was measured by the mean distance in inches which members of a pair maintained between themselves in the open field.

*Subjects and apparatus.* Eighty naive male albino rats of the Sprague-Dawley strain were housed in individual cages on an ad-lib food and water schedule for the duration of the experiment. Animals arrived in the laboratory at 26 days of age and testing began at 40 days of age.

The apparatus was a circular wooden open field (Latané, 1968), 4 feet in diameter, with an 18-inch surrounding wall. The field was painted glossy white, and the floor marked with black lines into 49 sections of equal area and approximately equivalent shape.

*Experimental conditions.* Animals in the *preexposure* condition ($n = 34$) were removed from their home cages and placed alone in the open field for 5 minutes a day for 7 consecutive days. Animals in the nonpreexposure condition ($n = 46$) did not receive experience in the open field. Some of these animals ($n = 16$) received 7 days of handling, during which they were picked up each day and carried into the testing room just as in the preexposure group, but were placed in an empty cage for 5 minutes rather than in the open field. The others ($n = 30$) received no prehandling. Since prehandling had no significant or detectable effect on subsequent emotionality or gregariousness, these groups were combined.

*Test trials.* Rats were tested in pairs for 5 minutes per day over 7 consecutive days, starting on the day after the last preexperience trial. Both partners were preexperienced for 12 pairs of rats, 18 pairs were composed of two rats who had not been preexposed, and 10 pairs consisted of one preexposed and one nonpreexposed rat. After being placed in the open field with their permanent partner, rats were allowed to roam freely about the open field for the next 5 minutes. Observers recorded the locations of the rats at 10-second intervals. At the end of 5 minutes, the rats were removed, fecal boluses were counted, and the field cleaned with a damp sponge.

*Retest trials.* After the testing week was completed, 48 rats representing all experimental conditions were divided into four groups and reexposed to the open field with their original partners for 5-minute periods at either weekly ($n = 6$), biweekly ($n = 18$), or monthly ($n = 12$) intervals for the next 2 months. A final group ($n = 12$) was retested only once, 2 months after the last test.

*Measures.* Gregariousness was measured by the average distance in inches which the rats maintained between themselves. Distance scores were calculated on a computer[5] from the recorded locations of each rat and averaged over the 30 observations in each test session. Distance scores could range from 0 to 40 inches, with lower scores indicating greater affiliation. Since "freezing" is a common response of rodents to frightening situations, the number of consecutive 10-second intervals during which the rat remained in the same square was taken as a measure of fear. A second measure of fear was whether the animals defecated in the open field.

## RESULTS

### *Effect of Being Together in the Field on Emotionality*

Fifty-six percent of naive subjects placed alone in the open field defecated during the preexposure week, compared with only 20% of naive nonpreexposed animals placed in the same unfamiliar environment in pairs during the testing week ($p < .005$ by chi-square).[6] Animals in the former group were immobile for an average of 23% of the recording periods, compared to only 9% for animals in the latter group ($p < 0.005$ by $t$ test). These observations confirm the findings of Latané (1968) and Latané and Glass (1968) and suggest that the presence of a partner in the open field reduced fear.

### *Effect of Familiarity with the Open Field on Emotionality*

Twenty-three percent of the nonpreexposed subjects defecated during the testing week, compared to only 3% of those who had had a week of preexposure ($p = .05$ by Fisher's exact test). No rat defecated during the last three days of the testing period. Although immobility was low in all conditions, these defecation data suggest that as the animals became acquainted with the previously unfamiliar environment of the open field, their emotionality decreased.

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[5] The computer program, RATLUV, was developed by Virginia Joy.

[6] All $p$ values reported in this paper are based on two-tailed tests.

### *Effect of Familiarity with the Open Field on Gregariousness*

Analysis of variance of the distance scores for the three groups over days showed significant differences due to Day of Testing ($F = 32.38$, $df = 6/222$, $p < .0005$), Prior Experience ($F = 36.13$, $df = 2/37$, $p < .0005$), and to the Day of Testing × Prior Experience interaction ($F = 6.58$, $df = 12/222$, $p < .0005$).

The uppermost curve in Figure 1 shows distances maintained by pairs where neither partner was familiar with the open field. This curve is very similar to those obtained in previous experiments for rats run under identical circumstances. The overall mean for the first day of testing was 17.9 inches, smaller than the approximately 25 inches which would be obtained by chance (Latané, 1968). Sixteen of the 18 pairs of rats stayed closer together than chance on the first day of testing ($p < .01$ by sign test). The average daily distance for this group decreased regularly over the 7-day period until on Day 7 it was only 6.7 inches, less than half of the distance maintained on the first day. Each of the 18 pairs of rats stayed substantially closer on the last day of the experiment than on the first ($p < .01$ by sign test).

The bottom curve in Figure 1 is dramatically different. It represents the distance maintained between partners both familiar with the field. The mean distance between these animals on their first day together was only 4.8 inches, over 13 inches closer than their counterparts for whom the field was novel. There was no overlap between the scores of rat pairs in the Preexposure and the Nonpreexposure conditions until the fourth day of testing. Although 10 of the 12 preexposure pairs decreased in distance from first to last day of testing ($p < .05$), the actual increase in gregariousness was small.

Rat pairs in which one animal was familiar and one unfamiliar with the open field show an intermediate level of affiliation. Each of the 10 pairs in this group increased in gregariousness from the first to the last day of testing ($p < .01$).

Although the three curves obviously approached each other as the test week progressed, there were still significant differences among them on the last day ($p < .01$ by Kruskal-Wallis).

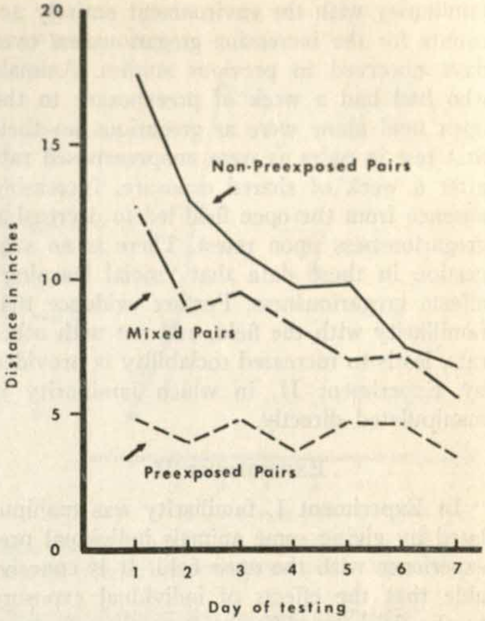

FIG. 1. Social attraction by day of testing—Experiment I. (Higher distances indicate *lower* attraction.)

### *Effect of Passage of Time on Gregariousness*

When a week or more was allowed to elapse between exposures to the open field, a marked loss of gregariousness was apparent. Twenty-two of the 24 retested pairs showed an increase in distance from their final daily trial to their retest 2 months later ($p < .001$ by $t$ test). This loss of gregariousness was not significantly affected by the number of distributed trials which intervened.

The loss in gregariousness slowly increased over the 2-month period. The loss for rats run 2 weeks after testing was significantly less than that of the same rats run after 8 weeks ($p < .01$ by $t$ test). The comparable difference for animals run 4 and 8 weeks after daily testing approached significance.

## DISCUSSION

These data support the hypothesis that rats are more gregarious in familiar than in unfamiliar surroundings, and that increasing familiarity with the environment entirely accounts for the increasing gregariousness over days observed in previous studies. Animals who had had a week of preexposure to the open field alone were as gregarious on their first test in pairs as were nonpreexposed rats after a week of shared exposure. Increasing absence from the open field led to decreasing gregariousness upon retest. There is no suggestion in these data that "social learning" affects gregariousness. Further evidence that familiarity with the field, and not with other rats, leads to increased sociability is provided by Experiment II, in which familiarity is manipulated directly.

## EXPERIMENT II

In Experiment I, familiarity was manipulated by giving some animals individual preexperience with the open field. It is conceivable that the effects of individual exposure to the field are different from the effects of paired exposure. There is evidence in Experiment I, as in previous studies, that animals are less fearful when exposed to the field in pairs. It is also possible that rats are more likely to display territorial proprietorship when exposed to the field alone than when together with other animals from the start. Although it is unclear what these possibilities might lead to, a second experiment was designed to rule out alternative explanations stemming from them. In this experiment, rats were first tested in pairs in one open field for 4 days, and then were switched to a different open field for an additional 4 days of testing. If the daily increases in gregariousness observed in previous experiments were due to social learning or to extraneous experience, one should expect a continuous increase in gregariousness over the 8 days of the experiment. If, as hypothesized, increases in sociability are due to increasing familiarity with the open field, one should expect increasing gregariousness in the first 4 days, a drop in social attraction with the switch in fields, and a subsequent increase in the new open field.

Although two open fields could be designed to differ along any of a number of dimensions, it was decided to vary color and illumination. One field was black and dimly lit; the other, as in Experiment I, was white and brightly lit. On the basis of previous findings, two alternative predictions concerning affiliation in a black versus a white field can be made.

The evolutionary forebears of the laboratory rat are nocturnal, burrowing animals who do not normally expose themselves to bright light (Barnett, 1963). The pink-eyed albino rat seems especially inhibited by bright light. Richter (1922) found that activity in the laboratory rat was much greater in dark than in light surroundings. It is possible that the high level of social activity that rats exhibit in a white open field would be even higher in a black, dimly lit field.

On the other hand, several studies show that rats find strong illumination fearsome (Flynn & Jerome, 1952; Hefferline, 1950; Keller, 1941). (This might explain its depressive effect on general responding.) Other studies show that affiliation leads to fear reduction (Davitz & Mason, 1955; Latané, 1968; Latané & Glass, 1968). It may be that the opportunity to reduce fear is one motive underlying rat gregariousness. If so, one should expect that rats would be most fearful, and consequently most gregarious in the white open field.

### METHOD

*Subjects and apparatus.* Forty-eight naive male Sprague-Dawley rats were approximately 60 days old at the start of the experiment. They were randomly paired and housed with their testing partner in single cages with ad-lib food and water.

Two open fields were used in this experiment. One was identical to that used in Experiment I; the other differed only in that it was painted flat black with white markings, appearing like a "negative" of the standard white field. The white field was brightly lit by six overhead fluorescent fixtures and a 150-watt bulb hung 28 inches above its center. The black field was lit only by a 25-watt red bulb.

*Procedure.* Twelve pairs of animals were observed for 4 consecutive days, 5 minutes a day, in the white open field and on the fifth day were switched to the black open field for an additional 4 days. Twelve pairs of rats followed the reverse sequence. Two new measures were taken in this experiment: time in contact, and movement. "Contact" was defined as actual physical contact between the two animals, and was measured simply by a stopwatch. "Movement" was derived by a computer from the recorded locations of each individual rat; it is the average distance in inches between the successive locations of a rat at 10-second intervals.

## RESULTS

### *Effect of Color and Illumination on Fearfulness*

Rats defecated an average of 1.5 boluses per day in the white field compared to 1.2 boluses in the black field ($F = 3.26$, $df = 1/160$, $p = .08$). They were immobile 20% of their time in the white field and only 14% of their time in the black ($F = 14.8$, $df = 1/160$, $p < .001$). Their average movement in the white field was 15.8 inches; in the black field it was 17.8 inches ($F = 25.0$, $df = 1/160$, $p < .001$). On the whole, then, rats were more active and less fearful in the black field than in the brightly lit white field.

### *Effect of Familiarity on Fearfulness*

Over days, rats dropped fewer boluses (2.1 in the first half, .9 in second, $F = 87.4$, $df = 1/160$, $p < .001$). They became less immobile (21% versus 13%, $F = 28.8$, $df = 1/160$, $p < .001$) and moved greater distances (15.3 inches versus 18.4 inches, $F = 57.3$, $df = 1/160$, $p < .001$). These data suggest that as the rats became familiar with the fields they became less fearful. There was, however, no effect of the switch of fields on any of these three measures. Changes between Days 4 and 5 were in the same direction and of the same magnitude as changes between other days.

### *Effect of Color and Illumination on Gregariousness*

Contrary to both of the possibilities suggested above, the color and illumination of the open field had no effect on gregariousness. Rats spent trivially more time in contact in the white field than in the black (44% of the time versus 41%, *ns*), but stayed trivially closer together in the black field than in the white (14.4 inches versus 15.5 inches, *ns*). In one respect, these results are encouraging. They suggest that specific characteristics of the open-field testing situation do not markedly influence sociability, and that the generality of results obtained in previous experiments may not be limited to fear-inducing test environments.

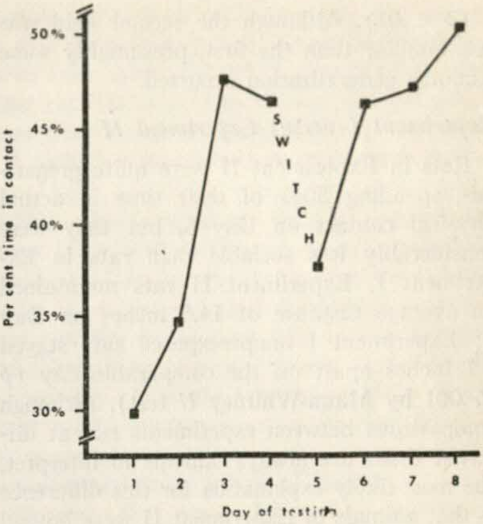

FIG. 2. Social attraction by day of testing—Experiment II. (Higher contact indicates greater attraction.)

### *Effect of Familiarity on Gregariousness*

As in Experiment I and in previous studies, rats became more gregarious over time. In the first open field, 21 of 24 pairs of rats spent more time in contact on Day 4 than on Day 1 ($p < .01$ by sign test); 18 pairs stayed closer together ($p < .05$). The same pattern of results showed up in the second field: 20 of 24 pairs spent more time in contact on Day 8 than on Day 5, and 22 pairs stayed closer together (both $p$'s $< .01$). Rats also spent more time in contact over the second 4-day period than over the first ($F$s $= 10.0$ and $10.3$, $df = 1/160$, $p < .001$).

The result of the switch in fields between Days 4 and 5 confirms the hypothesis that familiarity with the field is the important variable. There was a noticeable drop in gregariousness (Figure 2). Rats spent 46% of their time in direct physical contact on Day 4, compared to only 38% of their time on Day 5 ($p < .01$). The average distance between them rose from 14.5 inches to 16.6 inches ($p < .05$). Nineteen of the 24 pairs showed a decrease in physical contact and 18 showed an increase in distance between the 2 days.

Although gregariousness was significantly lower on Day 5 than on Day 4, it was still higher than on Day 1; 20 of 24 pairs spent more time in contact on Day 5 than on Day

($p < .01$). Although the second field was ess familiar than the first, presumably some timulus generalization occurred.

*Experiment I versus Experiment II*

Rats in Experiment II were quite gregarious, spending 50% of their time in actual hysical contact on Day 8, but they were onsiderably less sociable than rats in Exeriment I. Experiment II rats maintained n average distance of 14.5 inches on Day ; Experiment I nonpreexposed rats stayed .7 inches apart on the comparable day ($p < .001$ by Mann-Whitney *U* test). Although omparisons between experiments run at diferent times are always difficult to interpret, he most likely explanation for this difference s that animals in Experiment II were housed n pairs, while animals in Experiment I were oused individually. Schneider (1967), Capell and Latané (1969), and Latané, Cappell, nd Joy (1968) have shown that social isoation has powerful effects on open field ociability.

## DISCUSSION

Previous investigations of social attraction n rats have often used experimental situaions in which one rat is given the opporunity to approach another rat enclosed beind a wire mesh screen (Antonitis & Sher, 1952; Bayroff, 1936; Casey, 1962; Locke, 1936; Tolman, 1961). The amount of time pent in front of the compartment containng the stimulus rat has been the measure f sociability. The major difficulty with this echnique is that rats are not very much ttracted to each other if they cannot interct or have physical contact (Latané, 1968). s the present experiments show, when rats re permitted interaction and physical conact they are very much attracted to each ther.

The use of free-moving rats and a meaure of distance or contact does involve one roblem, however. Rats may stay close toether or remain in physical contact not beause they are attracted to each other, but ecause they have been independently atracted to some intrinsically desirable locaion. Congregation of animals around a waer hole in the desert may be motivated in part by social needs, but it can also be explained in simple terms of thirst. This possibility is particularly important in the present experiment, since it may require time and experience in the field for rats to discover the most desirable locations. It may be that increased gregariousness over days reflects learning and increased consensus as to the most pleasant area of the field, rather than increased social attraction.

In order to counteract this problem, the fields were constructed to be as homogeneous as possible. They were circular so that rats would not be independently attracted to corners. They were evenly lit (or unlit) so that rats would not be independently attracted to dark areas. They were swabbed down thoroughly between each trial so that rats would not be independently attracted to (or repelled from) the odor traces of previous occupants.

The results of these and previous experiments in this series suggest that these precautions were successful. Although rats did tend to stay near the wall (they spent over 95% of their time in the outer 50% of the open field), they also tended to move around, showing no preference for any one location (rats moved from one location to another during almost 90% of all 10 second recording intervals). More conclusively, using the same apparatus, Latané (1968) tested individual rats in the open field, but calculated distance scores *as if* two rats had been tested simultaneously. These "chance" distance scores would reflect any tendency for rats to independently seek one desirable location, without, of course, showing the effects of social attraction. This chance distance averaged about 25 inches (much greater than that exhibited by actual pairs of rats) and did not change over days. These considerations suggest that the present results cannot be attributed to independent location seeking. The authors think they can be explained in terms of rat's tendency to explore novel or unfamiliar environments.

Over the past 15 years, numerous psychologists have demonstrated the importance of exploration, either as a drive or as a potent response to novelty (Berlyne, 1960; Fowler, 1965). Animals placed in a novel environment explore it, often to the exclusion of other activities. For example, rats deprived of food for as much as 72 hours spend only a small percentage of time eating during the first minutes of exposure to a new environment (Zimbardo & Montgomery, 1957). Welker (1959) reported that hungry animals placed in a novel environment containing food do not contact the food as quickly as animals tested in their home cages. The failure of food-deprived animals to eat in a new environment suggests that exploration can compete successfully with other, usually prepotent, responses. The results of the present study are consistent with the view that exploration can also compete successfully with affiliation in a new environment.

Systematic investigations of exploratory behavior reveal that the amount of exploration decreases regularly as responses to a novel environment habituate with repeated exposure (Berlyne, 1950; Montgomery, 1955). Exploration recovers from habituation after a period of absence from the environment (Butler, 1957; Glanzer, 1958; Welker, 1961). The decrement in exploratory behavior produced by exposure to one environment generalizes to other, similar environments, but the magnitude of this generalized decrement decreases as similarity decreases (Montgomery, 1953). If we assume that exploration competes with affiliation, these effects can completely explain changes in affiliation observed in the present experiments. Rats became more affiliative over days as exploration presumably decreased; they became less affiliative after absence from the field as exploration presumably recovered; a change in the testing environment led to a partial but not complete drop in affiliation as exploration presumably increased in a partially novel situation.

What motivates exploration in the open field? A "curiosity" explanation suggests that exploration reflects a positive attraction to novelty. It may be that rats are "interested" in and seek out novel stimuli. On the first days of exposure to the field, they will be interested in and seek out both the other rat (rats in both experiments stayed significantly closer together than chance even on the first day of testing) and other, nonsocial stimuli. With continued exposure, according to this line of argument, responses to nonsocial cues habituate, but the response to the other rat does not. This leaves the rats with more time to devote to each other.

A second explanation for exploration in an open field has been advanced by Welker (1959), who suggested that in situations where all stimuli are novel, behavior which would seem to be novelty approaching may really be escape oriented. According to this view, activity in the open field is due to fear and the seeking of escape routes, rather than to curiosity. With the passage of time, fear, which might distract the animals from showing much interest in social interaction, should decrease, leading to greater affiliation.

Whether the manipulations of environmental familiarity in the present studies have affected fear, curiosity, or both, cannot be determined from the present data. Whichever is the case, however, it is clear that rats are much more sociable when they are on familiar ground. Although results with rats can at present serve as little more than suggestive analogies to human phenomena, there is little reason to think that processes such as those suggested above could not also apply to man.

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# ATTRACTION AS A FUNCTION OF THE COMMONALITY AND DESIRABILITY OF A TRAIT SHARED WITH ANOTHER[1]

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An experiment tested the hypothesis that when a desirable trait is shared with another, attraction will be greater when the trait is uncommon within the general population than when it is common, but when the trait is undesirable, attraction will be greater when the trait is common rather than uncommon. College women evaluated another woman after learning they were similar to her in ability to discriminate tastes. Some subjects were told they were very good at discriminating tastes; others, that they were very poor. Some were informed that 90% of the general population had the same level of ability as themselves in discriminating tastes; others, that 10% of the population had the same ability. The results supported the hypothesis.

Of the numerous factors that have been shown to affect interpersonal attraction, the effects of attitude or trait similarity have probably received the most research. Byrne and his associates (Byrne & Griffitt, 1966; Byrne & Nelson, 1964, 1965a, 1965b; Byrne & Rhamey, 1965) have consistently found a positive linear relationship between the proportion of similar attitudes and interpersonal attraction. There have been relatively few studies that have investigated parameters of the shared attribute-attraction relationship. Given that an attribute is shared with another, so that similarity is held constant, how does the nature of the shared attribute affect attraction? Newcomb (1956) suggested that the importance of the attribute has a significant effect on attraction, but experimental investigations have indicated that this suggestion may be valid only under limited conditions (Byrne & Nelson, 1964, 1965b; Clore & Baldridge, 1968).

Another parameter that would seem to be relevant to the shared attribute-attraction relationship is the commonality of the attribute, or the number of people who also possess the attribute. Would a person who shares an attribute with another like the other more when a large number of persons in the general population also have the attribute or when only a small number of persons have the attribute?

In general, it would seem that when a small number of people have an attribute, a person would feel psychologically closer to, or in a unit relation with, a particular other who also has the attribute than when a large number of people have the attribute. As Heider (1958) said,

> Whether two entities are seen as making up a unit depends not only on their relation to each other—whether they are similar or close to each other—but also on the properties of the surrounding. This can easily be demonstrated in figural perception. If two crosses are surrounded by circles, they will form a strong unit as a pair; if they are surrounded by other crosses, the pair unit will not be perceived. [p. 178]

When many people possess a trait, the person should be much less likely to conceive of himself in a unit relation with another than when very few people possess the trait. If the shared attribute is socially desirable, then the person should be pleased to know he possesses it. If few people possess the desirable attribute, then the other should be strongly associated with the person himself and his pleasant state and should be viewed as pleasing and attractive. When many people possess the desirable attribute, the other should not be associated with the person and his pleasant state and liking for the other should not be increased.

On the other hand, if the attribute were *undesirable* and few people possessed it, then the other would be associated with, or linked to, the person and his unpleasant state and should be viewed as unpleasant and unattractive. When many people possess the undesirable trait the other should not be linked to the person and his unpleasant state, so liking for the other should not be decreased. On the basis of these notions, it might be expected that when the attribute shared with another is desirable the effect of the commonality of the trait on attraction might be the opposite of when the trait is undesirable.

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Given that a person is similar to another, in that they both possess a trait, the above reasoning provides some basis for inferring how the commonality and desirability of the trait might affect attraction. Specifically, it suggests that when the shared attribute is desirable the other will be liked more when few people have the attribute than when many have it, but when the shared attribute is undesirable the other will be liked more when many people have the attribute than when few people have it.

If it is readily possible to make inferences about additional attributes from the knowledge that the other possesses a particular attribute, then the person might infer that the other is more similar when the particular attribute is uncommon than when it is common. The perception of greater similarity might therefore mediate any differences in attraction when the shared attribute is uncommon rather than common. However, if it is very difficult to make inferences about additional attributes from the knowledge that the other has a particular attribute, then it would be difficult to interpret differences in attraction in terms of assumed similarity. Therefore, in the experiment described below a fictitious trait was selected which the subjects obviously did not know that they possessed and which provided relatively little information about other attributes.

## METHOD

The subjects were 33 undergraduate females at the University of Texas who participated in the study in order to partially fulfill an introductory psychology course requirement. They were assigned to blocks of four conditions in an unbiased manner.

Subjects who had signed up for a study of taste and smell reported individually to a room, the door to which was lettered with the following: *Olfactorium Taste and Smell.* Attached to the door was a paper sign indicating that the subjects for the taste and smell study were to wait in the hall. After a subject arrived at the door and read the sign, she was approached by a female experimental accomplice,[2] who had been standing in the hall reading a book. The accomplice asked the subject if she was to be in the taste and smell study, and then she said that a few minutes earlier the experimenter told her there was a slight delay and that he would be with them shortly. The accomplice then continued reading her book and avoided conversing with the subject. Within a few minutes the experimenter came out of the olfactorium and said things were running a little behind schedule. He explained there were some preliminary instructions and tests that he had to give both the subjects, that this could be done in another room, and then they would return to the olfactorium. He then took the actual subject and the supposed subject to a small room in the building.

The experimenter told the subjects that the purpose of the study was to see whether people who could make fine discriminations in one sense modality were also good discriminators in another sense modality. The subjects were told that there was a simple test that indicated which people were good discriminators in the sense of taste, but that testing for the ability to discriminate different odors was much more difficult and would necessitate using the olfactorium. Next, the experimenter explained that before administering the taste sensitivity test he would like them to read a booklet. The experimenter said that the booklet described the taste test, and, in conjunction with the psychology department's policy of increasing the educational benefits of participating in experiments, the booklet pointed out some of the practical implications of the taste test. The subjects were given a booklet and told to read it carefully while the experimenter set up the materials for the taste test.

The booklet, approximately 350 words in length, contained the information designed to manipulate the independent variables. The booklet began with a statement that differences in taste-discrimination ability had been shown to be related to the presence or absence of phenylthio-renin (PTR) in the saliva. It stated that the trait of having PTR in the saliva is inherited. In the high-percentage condition the booklet stated that research over the past 20 years indicated that about 90% of the population had PTR in their saliva. In the low-percentage condition the booklet stated that about 10% of the population had PTR in their saliva.

The next paragraph contained the information describing the supposed desirability of the trait of having PTR in the saliva. In the positive-desirability condition the booklet stated that PTR acted on the taste buds as a chemical *sensitizer,* and that the people who had PTR in their saliva could make subtle discriminations in taste which were impossible for persons without PTR in their saliva. The remainder of the paragraph in the desirable condition was as follows:

> Not only is the trait important to gourmets and professional cooks, but the average housewife who

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[2] Fran Reneau deserves thanks for performing this role.

possesses the trait can easily make the taste discriminations necessary to do a really good job of everyday cooking. What is more, in comparison to a counterpart without the trait, the person who has this discrimination ability is much more likely to become a truly fine cook and has the potential of becoming a gourmet.

In the negative-desirability condition, the booklet stated that PTR acted as a chemical *desensitizer,* and that the people who had PTR in their saliva could not make subtle discriminations in taste which were possible for persons without PTR in their saliva. In the undesirable condition the booklet continued:

> Not only is the absence of the trait important to gourmets and professional cooks, but the average housewife who possesses the trait cannot make the taste discriminations necessary to do a really good job of even every day cooking. What is more, in comparison to a counterpart without the trait, the person who lacks this discrimination ability is much less likely to become a truly fine cook and does not have the potential of becoming a gourmet.

The last paragraph of all the booklets described the test for PTR in the saliva as follows:

> "A small amount of magnesium oxide (usually mixed with an artificial sweetener to mask the taste of the reaction with PTR) is applied to the tongue." The powder is then mixed with the saliva and then the indicator paper is wetted with the saliva. "If the paper turns from pink to purple the trait is present; if it remains rose the trait is absent."

A footnote was added at the end of the booklet indicating that the material was taken from a recent book on heredity.

Since it was necessary for the subject and the accomplice both to have the fictitious trait, the test was designed so that the test paper would always change from pink to purple. This was accomplished by mixing granulated peppermint candy with the magnesium oxide and employing test paper which is sensitive to sugar. When the powder mixture was diluted there was a high concentration of sugar in the saliva and this acted to change the color of the chemically impregnated test paper.

The subjects then took the taste test, and the experimenter instructed them to hold the test paper in front of them after completeing the taste test. He took the taste papers from the accomplice and the subject and placed them on a table in clear view of the subject and commented, "I see that both of you have the trait," while noting the results in the taste test on a record sheet.

The experimenter then explained that although the test for taste discrimination was very simple the test for odor discrimination was much more difficult because subtle factors such as a person's mood could influence her ability to discriminate odors. Since mood could affect the smell test it was necessary to know the subject's mood and feelings as she was about to enter the olfactorium. The experimenter then gave the subject and the accomplice a booklet labeled "Subjective Feeling Index." The subjects were told that the booklet contained questions concerning their feelings that would help in interpreting the results of the smell test. The booklets were identified only by number and the subjects did not have to sign their names to them. The experimenter requested that when they had completed the questionnaire that each be placed in separate manila envelopes which were addressed to a secretary in the psychology department. He explained that the secretary did all the tabulating of the questionnaires, and that he never saw them. The experimenter then excused himself, saying that he would check if things were ready in the olfactorium while they completed the questionnaire.

The "Subjective Feeling Index" began with questions concerning whether the subject had tasted any highly seasoned foods or had been exposed to any strong odors recently. Following two items on which the subject was to rate how anxious and rested she felt, there were three items to assess interpersonal attraction. The first was: "How much do you like the other subject in this study?" This item was answered on a scale from 0 (a little) to 20 (very much). On the second and third items the subject indicated on a 21-point scale how likable the other subject was and how annoying or pleasing she was. These items were followed by several items, all on 21-point scales, concerning the similarity of the other subject, the likability and pleasantness of the experimenter, and the desirability of making taste and smell discriminations.

After allowing time for the completion of the questionnaire, the experimenter returned and indicated that they were ready for one person in the olfactorium and asked the accomplice to report there. The subjects was then asked a series of questions to check for suspicion. The comments of one subject indicated suspicion about the nature of the study. This subject was not included in the analysis of the results. Finally, the experimenter revealed the deceptions and real purpose of the study and asked the subjects not to discuss the study with anyone.

## RESULTS AND DISCUSSION

Since the experimental hypothesis dealt with the effects of various factors on attraction when similarity is relatively constant, it was important that the accomplice be perceived as equally similar to the subjects in all four experimental conditions. A check on the perceived similarity of the accomplice was provided by the ratings on the question, "How similar to yourself is the other subject?" The means for each condition for this measure are presented in Table 1. It can be seen from Table 1 that the other was rated virtually equal in similarity in all four experimental

TABLE 1

MEANS OF RATINGS OF SUBJECTIVE FEELINGS

| Desirability | % | Similarity of other | Attraction to $E$ | Anxious | Desirability of making fine taste discriminations |
|---|---|---|---|---|---|
| Positive | High | 7.3 | 13.3 | 9.0 | 14.1 |
| Positive | Low | 7.9 | 13.4 | 9.1 | 12.8 |
| Negative | High | 7.8 | 15.1 | 4.1 | 16.7 |
| Negative | Low | 7.8 | 14.6 | 6.1 | 13.9 |

conditions. An analysis of variance of these data showed that there were no significant treatment effects. The ratings of similarity support a basic assumption of the experimental design, that similarity be held constant among the experimental conditions. With this assumption satisfied, attraction, the dependent variable, can be examined.

Attraction was measured by the average of the rating on the item "How much do you like the other subject in this study?" and the ratings of likability and the pleasantness of the other subject. It should be noted that the ratings on each of these items separately followed essentially the same pattern as the summed measure. The means of the attraction scores for the experimental conditions are presented in Table 2. Examination of Table 2 shows that for the positive-desirability condition the other was liked more in the low-percentage condition than in the high-percentage condition, but in the negative-desirability condition the other was liked more in the high-percentage condition than in the low-percentage condition. An analysis of variance, which is summarized in Table 3, indicated that the interaction between desirability and percentage was significant beyond the .02 level. Neither the effect of desirability nor the effect of percentage approached significance. The means of the attraction ratings follow the predicted pattern and provide support for the hypothesis.

TABLE 2

MEANS OF ATTRACTION RATINGS OF THE OTHER

| Desirability | % | |
|---|---|---|
| | High | Low |
| Positive | 9.42 | 11.79 |
| Negative | 12.00 | 8.83 |

Note.—$n = 8$ in all conditions. The larger the number, the greater the attraction.

TABLE 3

ANALYSIS OF VARIANCE OF ATTRACTION RATINGS

| Source | $MS$ | $F$ |
|---|---|---|
| Desirability (D) | .28 | <1 |
| Percentage (P) | 1.26 | <1 |
| D × P | 60.21 | 7.21** |
| Error | 8.35 | |

** $p < .02$, $df = 1/28$.

Looking at the results from another angle, it can be seen that when a small percentage of people have a trait the other is liked more when the trait is desirable than when it is undesirable ($t = 1.78$, $df = 28$, $p < .10$).[3] However, when a large percentage of people have a trait the other is liked more when the trait is undesirable than when it is desirable ($t = 2.05$, $df = 28$, $p < .05$). Intriguingly, the other in the high-percentage–undesirable condition was liked as much as the other in low-percentage–desirable condition. This finding was unexpected and is difficult to explain.

It might be thought that the high degree of liking for the other in the high-percentage–undesirable condition could be explained by assuming that when a large percentage of people have a trait, it is no longer evaluated as especially desirable or undesirable. According to this reasoning, the high-percentage–negative desirability condition actually represents a "high-percentage–neutral-desirability" condition and the high-percentage–positive desirability condition also represents a "high-percentage–neutral-desirability condition." Therefore, logically there should not have been any difference in liking between the high-percentage–positive-desirability condition and the high-percentage–negative-desirability condition, but there was. Additional evidence which may bear on the tenability of this assumption is provided by the ratings for the question, "How desirable do you think it would be to be able to make fine taste discriminations?" From Table 1, which shows the means of the ratings for this question, it can be seen that the desirability of making fine taste discriminations was rated slightly higher in the negative-desirability conditions than in the positive-desirability conditions and slightly higher in the high-percentage conditions than in the low-percentage conditions. Neither of these effects nor the interaction was significant by analysis of variance. There is a significant tendency, however, for subjects in the negative-desirability–high-percentage condition to rate the desirability of making fine taste discriminations higher than subjects in the other three experimental conditions ($t = 2.08$, $df = 28$, $p < .05$). Possessing a negative trait and knowing that only a few people possess the positive trait seems to make possession of the positive trait much more desirable and in part implies that in the high-percentage condition the negative trait was undesirable. In sum, the tenability of the assumption that the present experimental manipulation of increased percentage possessing the trait also manipulates the trait's desirability towards neutral seems questionable on logical and empirical grounds.

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[3] Statistical significance determined with two-tailed $t$ tests.

It is possible that the experimental manipulations affected the tendency of subjects in the different conditions to like or dislike everyone, not just liking for the other subject who had a similar trait. That is, perhaps subjects in the different conditions felt happy or disappointed as a result of learning about their taste-discrimination ability and this affected their evaluations of everyone. The confidential ratings of the likability and pleasantness of the experimenter provide a check on this interpretation. The means of the average of the ratings of the likability and pleasantness of the experimenter for the experimental conditions are given in Table 1. The means of attraction to the experimenter for the four experimental conditions do not follow the interaction pattern of the means of attraction to the other subject. There is a nonsignificant tendency for the experimenter to be liked more in the negative-desirability conditions than in the positive-desirability conditions. Thus, it seems that the experimental manipulations did not have a general effect on liking for others but were specific to liking for the subject who shared the characteristic.

The only other item on the Subjective Feeling Index that showed any significant effects in the analysis of variance was the question dealing with how anxious the subjects felt. This item was on a scale from 0 (not at all) to 20 (extremely). From the means of the ratings for this item, which are presented in Table 1, it can be seen that the ratings are higher in the positive-desirability conditions than in the negative-desirability conditions. Analysis of variance confirmed that the difference between the desirability conditions was significant ($F = 5.98$, $p < .05$). Neither the effect of percentage nor the interaction was significant. Upon first consideration it might seem that when a person learns that he possesses a desirable trait he would feel satisfied and relaxed, while the knowledge of the possession of an undesirable trait would induce dissatisfaction and anxiety. However, it may be that the subjects who were told they were very good in making taste discriminations thought they also would be expected to be good at odor discriminations. Concern about fulfilling this expectation may have aroused anxiety.

One factor that might help account for the relatively high amount of attraction to the other in the high-percentage–negative-desirability condition is the person's desire to avoid persons or situations that would make her undesirable attribute salient. A person who had the desirable attribute would be highly likely to increase the salience of the subject's undesirable attribute. The subjects in the high-percentage–negative-desirability condition might have been very relieved that the other subject had the undesirable rather than the desirable trait. As a result, liking for the other may have been increased. This interpretation suggests that in the high-percentage–negative-desirability condition the other reduced the subject's anxiety. The ratings of how anxious the subject felt provide only marginal support for this interpretation. Rated anxiety is significantly lower in the high-percentage–negative-desirability condition than in either the high-percentage–positive-desirability condition ($t = 2.15$, $df = 28$, $p < .05$) or the low-percentage–positive-desirability condition ($t = 2.20$, $df = 28$, $p < .05$), but it is not significantly lower than in the low-percentage–negative-desirability condition ($t < 1.0$). As noted above, however, the relatively high anxiety in the positive-desirability conditions can be accounted for on other bases.

In sum, the results support the hypothesis that when a person shares a desirable trait with another, the other is liked more when a small percentage of the general population also has the trait than when a large percentage also possesses it, but when the trait shared with the other is undesirable, the other is liked more when a large percentage of the population also has the trait than when a small percentage has it. Schachter (1959) summarized an experiment on the directionality of affiliation by changing the old saw "Misery loves company," to "Misery doesn't love just any kind of company, it loves only miserable company [p. 24]." In the present study, the difference in attraction between the high and low percentage conditions, when the shared trait was undesirable, suggests another qualification of this epigram, which is expanding in size but decreasing in scope: Misery loves miserable company when the world is full of miserable people.

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# ON BEING LIKED MORE THAN YOU LIKE[1]

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In one experiment, 52 subjects indicated how pleasant it would be if people whom they liked, at varying levels, reciprocated the liking at varying levels. Discomfort under conditions of cognitive asymmetry (a discrepancy between emitted liking and the perceived level of being liked) was generally verified: subjects indicated that they would be happiest not only when their friends liked them, but also when their enemies disliked them. However, no relative discomfort accompanied the small asymmetry of being especially well liked by a liked other. Results were verified in a second experiment which dealt with actual cases of perceived asymmetry. Subjects indicated that they would be happier if marked asymmetry were decreased rather than increased, even if this meant being less well liked rather than better liked. There is an actual strain toward symmetry within dyads; the expectation of symmetry is not simply a "best guess" based on past observation of the social world.

Almost anyone, it would seem, would be unhappy if his friends did not like him. But is a person apt to be happy or unhappy on discovering that a presumed enemy does not, in fact, dislike him? As another case, how does a person feel if he "likes Pat moderately well," and he suddenly sees that Pat "likes him a great deal"?

Group members, in a variety of settings, have been found to believe routinely that their liking for others will be closely reciprocated (Blumberg, 1967; Newcomb, 1961; Tagiuri, Blake, & Bruner, 1953). However, traditional sociometric procedures do not reveal whether the basis for the expectation is a "dispassionate best guess" at the truth, or whether perceived asymmetry will actually elicit feelings of discomfort—as would be predicted by theories of cognitive consistency.

According to both Heider's (1958) balance theory and Festinger's (1957) dissonance theory, one is expected to feel uneasy about unbalanced or dissonant situations and, thereby, to be motivated to strive for balance or consonance. In terms of balance, the self—a positive object—ought to be liked by positive others and disliked by negative others. Furthermore, it is implicit in DeSoto's (1960) view of a symmetry schema as a "cognitive good figure" that perceived deviation from the schema will be a source of discomfort.

DeSoto and Kuethe (1959) showed that the liking relationship is characterized by a symmetry schema—that, in the abstract, subjects expect positive liking to be reciprocated with positive liking and (in contrast with power relationships) more readily learn a hypothetical social structure if liking is indeed presented as symmetrical (DeSoto, 1960; DeSoto & Kuethe, 1959).

If there were no theoretical rationale to the contrary, one would be tempted to predict that the more we are liked, the happier we will be. The more an enemy likes us, the less he will harm us. The more a friend likes us, the more he will provide us with many kinds of rewards. According to this common-sense view, the sociometric and abstract expectations would be accounted for as follows: Group members tap a symmetry schema to make a purely intellectual decision that their liking is reciprocated—it is not that they routinely "prefer things that way." The symmetry schema, here, could simply be the result of induction based on observation. (According to Blumberg, 1967, and Tagiuri et al., 1953, mutuality of liking actually does exceed a chance base line at all levels of liking except possibly the lowest ones.)

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[2] The author wishes to thank Clinton B. DeSoto for advice in carrying out the first experiment and Gordon Becker for raising a question which eventually led to the second experiment. Requests for reprints should be sent to Herbert H. Blumberg, Psychology Department, Wilson College, Chambersburg, Pennsylvania, 17201.

In contrast with a purely "dispassionate best guess" view, Newcomb (1961) has suggested that there is also a "strain toward symmetry"—a delicate balance of social interactions, whereby the most rewarding social structures are perceived as symmetric and balanced. In his analysis of the acquaintance process, Newcomb indicated that the individual must simultaneously come to terms with the reality of related people and groups, with the world he has in common with them, and with autistic demands to reduce strain implicitly due to imbalance (Newcomb, 1961; Newcomb, Turner, & Converse, 1965).

Employing a role-playing task (using a method developed by Jordan, 1953), Price, Harburg, and McLeod (1965) demonstrated a strain toward symmetry in their finding that subjects felt least pleasant if they were "members" of unbalanced triads. The subjects' task was (*a*) to specify (by initials) people whom they actually liked or disliked with various intensities; and (*b*) to imagine, in each of a series of questions, that one of these people liked or disliked another person with some particular intensity. Subjects rated how pleasant or uneasy they felt about each situation (triad of self and two others). The subjects were given no explicit information about reciprocation of liking within the dyads making up each triad. In a subsequent study, Price, Harburg, and Newcomb (1966) showed that the direction and sign of particular bonds tempered the force toward balance, as when a negative link to the subject resulted in greater uncertainty about reciprocation or in indifference because the triad failed to "engage" the subject with someone he himself liked.

Several recent studies have shown that tension is associated with imbalance (and with other factors) in triadic relations that include an impersonal entity (Rodrigues, 1967, 1968; Taylor, 1968). In Taylor's study it was found that when balance or imbalance was experimentally created by having a liked or disliked other take a role of either agreeing or disagreeing with a subject's position on an issue, that tension (as measured by means of Bales' interaction process analysis) was initially greater for unbalanced situations (which also showed a greater loss of tension in the course of discussion between the two parties).

In Experiment I the investigator adopted the role-playing task used by Price et al. (1965), but applied it to symmetry of dyadic liking. Reciprocation within the dyad provides a particularly strong test of the force toward balance, because of the supposed clear presence of an antagonistic force ("the more someone likes me, the better"). Will people actually feel bad in supposing that someone likes them *more* than they thought (providing simply that the result is asymmetric)?

If it is found that discomfort is positively related to asymmetry of dyadic liking, this will suggest a strain toward balance. Otherwise, one would conclude that the previously demonstrated expectation of reciprocation is due to a "best guess" as to what actually occurs (or what Newcomb calls the forces of reality). The latter conclusion would be suggested regardless of whether there are no significant differences in discomfort (indifference, on the part of subjects) or whether there is a general preference for situations where the subject is well liked (an egoistic strain). Of course, subjects may differ as to which forces are strongest.

## EXPERIMENT I

### *Method*

Fifty-two male and female undergraduates, selected at random from an introductory psychology class, were asked to fill in the initials of four same-sex peers, as follows: best friend, friend whom you moderately like, person whom you slightly like, and person whom you dislike or like least. Then subjects were given the following instructions:

> Each of the next two pages contains a set of eight questions. On the first line of each question fill in the initials . . of the appropriate person. . . . On the last line of each question, *indicate by a check mark, anywhere along the line below each situation, the point which best shows how this situation makes you feel.* Put *one* check mark on the line below *each* question, even if you have to make a reasonable guess. The point is to *imagine* how you would react if you saw that a particular real person felt a certain way.
>
> As a sample question:
>
> I moderately like ______________.
> I see that this person *strongly likes* me.
> I feel: uneasy ________·________ pleasant

In an appropriately balanced sequence, the 16 questions included each of the four levels of liking, paired with each of the four levels of being liked. (Subjects were not asked about actual perceived reciprocation, since—as noted previously—such estimates are known typically to be symmetric.)

### *Results*

Each scale of comfort was divided into eight equal intervals, with each response being scored from 1 to 8.

Table 1 shows mean comfort as a function of liking and the perception of received liking, and Table 2 provides an analysis of variance for the data. The two main effects indicate that comfort shows an overall increase with greater perception of being liked and, to some extent, with greater emitted liking. The significant interaction effect is due entirely to increased comfort in those situations showing increased symmetry between liking and the perceived level of being liked, as follows:

When one looks at the rows of Table 1, one finds a strong relationship between comfort and symmetry. The maximum comfort for each row occurs at the point where liking matches the perceived level of being liked; and the greater the asymmetry, the less comfort there is.

At this point, note that—implicitly in the instructions—the neutral point between liking and disliking occurs between the first two levels of liking (or being liked) among the four levels. This "imbalance" in the liking continuum was preserved in order to divide the continuum into readily discriminable categories.

Turning one's attention to the columns of Table 1, one finds "comfort in symmetry" verified in a qualitative sense, but not completely in a quantitative sense; that is, comfort is greatest when positive liking is reciprocated by positive liking, and disliking by disliking; but the highest level of comfort does not always occur for that degree of positive received liking which exactly matches a given level of positive emitted liking. In the first column—emitted disliking—comfort indeed varies directly with the degree of reciprocated *dis*liking. For the remainder of the columns, where positive liking is emitted, comfort does increase with the perception of greater liking received. However, comfort does not much decline (nor increase) when the perceived level of being liked *exceeds* that of emitted liking. (One does notice a minute trend toward such a decline for the lowest level of positive liking, L2, at the highest two levels of being liked, P3 and P4.)

TABLE 1

MEAN COMFORT AS A FUNCTION OF LIKING AND THE PERCEIVED LEVEL OF BEING LIKED

| Perception of received liking | Emitted liking | | | | |
|---|---|---|---|---|---|
| | L1 | L2 | L3 | L4 | All levels |
| P4 | 3.08 | 5.31 | 6.31 | 7.29 | 5.50 |
| P3 | 3.81 | 5.56 | 6.25 | 4.79 | 5.10 |
| P2 | 4.15 | 5.29 | 4.50 | 3.62 | 4.39 |
| P1 | 4.25 | 2.88 | 2.35 | 1.54 | 2.76 |
| All levels | 3.82 | 4.76 | 4.85 | 4.31 | 4.44 |

Note.—Categories of emitted liking range from L1 (lowest level) to L4 (highest level); similarly, perception of received liking ranges from P1 to P4.

With increased emitted liking, the polarization of discomfort for varying levels of being liked increases: the first column in Table 1 shows a range of from 3.08 to 4.25, while the fourth column shows a range of from 1.54 to 7.29. At the lowest level of liking (first column of Table 1) there is ambivalence, across subjects, in the feelings of relative "comfort at being disliked" and "discomfort at being liked"; the interquartile ranges of the lower- and upper-left cells are 3.4 and 3.8, as compared with an average interquartile range of just over 2.0 for the other cells.

The relatively high variability within the two named cells makes it desirable to apply a single conservative test to the important comparison between them. In terms of fre-

TABLE 2

ANALYSIS OF VARIANCE FOR RATED DISCOMFORT AS A FUNCTION OF PERCEIVED ASYMMETRY OF DYADIC LIKING

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Liking (L) | 3 | 46.47 | 19.86* |
| Perception (P) | 3 | 304.56 | 100.52* |
| Subjects (*S*s) | 51 | 6.10 | |
| L × P | 9 | 91.01 | 26.30* |
| L × *S*s | 153 | 2.34 | |
| P × *S*s | 153 | 3.03 | |
| L × P × *S*s | 459 | 3.46 | |

* $p < .001$.

quency, 30 subjects preferred to be *disliked* by a disliked other (higher rating for the condition represented by the lower-left cell, L1-P1), while 15 preferred to be liked by the disliked other (higher rating for upper-left cell; $\chi^2 = 5.0$, $p < .05$; 7 subjects showed no preference).[3]

## EXPERIMENT II

Two variables—asymmetry and nonreality—were necessarily confounded in the first experiment because of the need for systematic investigation of all levels of liking and perceived reciprocation with all subjects. Discomfort was found to increase with gross asymmetry, but it is not clear whether the increase was due to asymmetry per se, or whether it was due to a disturbance of the status quo. The questions that dealt with symmetric situations probably described the situations which actually occurred, whereas the increasingly asymmetric situations probably required greater amounts of role playing.

In the second experiment, subjects were asked explicitly about others whose actual reciprocation is perceived as asymmetric.

For those cases in which the subject perceives his emitted liking as *exceeding* received liking, it is clear how he ought to feel if he should find out that the other person "really liked him more than he had thought." He should feel happy, for symmetry would be increased, and, at the same time, he would see himself as being better liked than he had thought.

But what about the situation in which the perceived level of being liked surpasses that of emitted liking? Would one be happier to find that received liking was overestimated (the new situation would be more symmetric) or underestimated (in the new situation, one would be better liked)?

### *Method*

In a brief anonymous questionnaire, 98 female undergraduates were asked, first, to "think of a person whom you believe to like you *more* than you like him or her," and, in the second half of the questionnaire, to "think of a person whom you believe to like you *less* than you like him or her."

TABLE 3

FEELINGS WHEN PERCPIVED LIKING FROM ANOTHER EXCEEDS OWN EMITTED LIKING (FREQUENCY OF CASES PER CATEGORY)

| Degree of asymmetry | Would be happier if liked: | | |
|---|---|---|---|
| | Less | (Same) | More |
| More than 2 points | 26 | 1 | 0 |
| 2 points | 11 | 12 | 14 |
| 1 point | 3 | 7 | 16 |
| 0 (i.e., more than 0, but less than ½ point) | 0 | 0 | 8 |

For each half of the questionnaire, subjects answered the following five questions:

1. Is this person of the same or opposite sex?
2. How much do you like this person?
3. How much does this person like you?
4. How would you feel if you learned that this person likes you *more* than you thought?
5. How would you feel if you learned that this person likes you *less* than you thought?

The last four questions were answered by means of an X placed on each of four 7-point linear scales. The first two scales ranged from "not at all" to "very much," and the remaining two ranged from "unhappy" to "extremely happy."

### *Results*

On the basis of the questions about degree of liking and of being liked, the intended validity of the directions was verified. Responses to the two questions were invariably in line with the instructions to "think of a person whom you believe to like you *more* (or *less*) than you like him or her."

The main results have to do with relative happiness under conditions of increased or decreased symmetry. For the person who "likes you *less* than you like him or her," virtually all subjects would feel happier to learn that the person likes them more than they had thought, rather than less.

In Table 3, subjects are classified according to their responses to questions about the person "who likes you *more* than you like him or her." The first row of the table is concerned with cases in which emitted liking was more than 2 points below the perceived level of being liked. Out of 27 cases, 26 subjects would actually prefer increased symmetry over being better liked, and none preferred to be better liked ($\chi^2 = 26$, $p < .001$).

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[3] In a replication of this finding, 11 out of 13 boys at summer camp (10–14 years of age) also showed greater comfort for the "lower-left" (L1-P1) situation than for the "upper-left" (L1-P4) one ($\chi^2 = 6.2$).

For asymmetry equal to 2 points, it was about equally likely for subjects to prefer being either better liked or less liked (of the 25 subjects who showed a preference, 11 represented only a 1-point difference on the rating scale).

Once asymmetry equaled less than 2 points, subjects showed a clear preference to be liked more, even at the cost of symmetry (24 cases of "more," 3 of "less;" $\chi^2 = 16.3$, $p < .001$).

The preference for reducing marked asymmetry held for both same-sex and opposite-sex dyads (the latter represented about half of the total cases, including 16 of the 26 cases in the upper-left cell of Table 3). Since most of the findings of preferred balance in dyads and triads have been based on same-sex peers, it is reassuring to find that the forces toward symmetry are also strong in opposite-sex dyads (for female subjects, at any rate). Moreover, the overall distributions for the two kinds of dyads were not very different from each other (and corresponding cells of both varieties involved about the same distribution of absolute levels of liking), so that it is clear that the results of the present experiment are not primarily due to the special case of girls wanting to avoid being chased by boys whom they dislike.

One may ask whether the differences among the rows of Table 3 are due to differences in emitted liking rather than only to differences in symmetry. As symmetry increases, the dyads necessarily involve cases in which liking occurs at a rather low level, so that increases in asymmetry are tied with decreases in emitted liking. The cases in the first row do span the entire available range of liking. While such cases are defined in such a way as to exclude the possibility of moderately or strongly positive emitted liking, the preference for being less liked—the important overall finding—holds for cases of clear disliking as well as for those nearer to the middle of the liking scale.

On the other hand, the remaining finding, that mild asymmetry is accompanied by a preference for increased received liking, was clearly based only on cases of at least moderately positive liking; of the 34 cases represented in the lower two rows of Table 3, all but 6 represent emitted liking at one of the top three levels of liking.

Finally, when analyses are based on what are essentially difference scores—how does each subject's response to the question about being liked *less* compare with that about his being liked *more*?—there is always the possibility that results are due largely to the effect of one or the other of the component questions (Cronbach, 1958). However, for the present results, such appears not to be the case. For virtually all of the 26 cases in the upper-left cell of Table 3, reactions to the "less" question were within the upper half of the happiness scale, and also, answers to the "more" question were in the lower half. Just the reverse was true for the 24 cases in the two lower-right cells.

## DISCUSSION AND CONCLUSIONS

Discomfort with marked dyadic asymmetry was generally verified, although in cases of positive liking—when the perceived level of being liked exceeded that of emitted liking—the desire to be still better liked was sufficient to counteract a small amount of asymmetry. The following two factors, together, account for the results of both experiments: (*a*) discomfort due to asymmetry and (*b*) the preference to be well liked by those whom one likes.

In the first experiment, at every level of being liked (or disliked), the greatest average comfort was to be found when emitted liking matched the level of being liked. The result is consistent with the view (*a*, aforementioned) that the sociometric expectation that liking will be closely reciprocated is not simply based on an intellectual decision, but, rather, that people are actively motivated to maintain their liking at a symmetrical level.

When the variables are interchanged, for a given level of emitted liking the greatest comfort occurs when there is a qualitative symmetry: subjects were happiest when liked by those whom they liked, and disliked by those whom they disliked. However, when emitted liking was at least slightly positive, there was no relative discomfort when the perceived level of being liked *exceeded* that of emitted liking. In the last three columns of Table 1, large gains in comfort accompany increments of being liked—up to the point where being liked equals liking. Both previously stated Factors *a* and *b*, above, favor such gains. Within these columns, once the perception of being liked begins to exceed emitted liking, Factor *a* favors a decrease in comfort because of increased asymmetry, while *b* favors an increase because one will be better liked. The predictable and actual result is that there are no large changes at all. It is Factor *b*, the preference to be well liked by those whom one likes, that accounts for the increasing internal polarization from the leftmost column of Table 1 to the rightmost one. The relative magnitude of Factor *a* is increased when there is a large, qualitative asymmetry, in which liking and disliking reciprocate each other.

It may prove helpful to describe briefly an underlying system that supports the established preferences for symmetry. The amount of attraction emitted and received by particular individuals and groups is known typically to remain rather constant over time, even though specific friendship bonds and their bases may change (Blumberg, 1967; Newcomb, 1961; Tagiuri, 1958). In terms of potential rewards, one's "limited resources of emitted liking" will be most profitably and comfortably allocated if they at least match the perceived distribution of liking received from others (so that one can reciprocate the received liking, thereby assuring its continued flow). However, to exceed received liking would be to waste one's resources (although, in the short run, a slight excess might yield a "pump-priming" benefit, whereby the other person would then increase his liking). Such an analysis is consistent with the results based on the various rows of Table 1.

If one takes one's liking as given (corresponding to a particular column of Table 1), as when one falls in love or dislikes a bully, what kind of reciprocation would be most satisfying? When received liking is seen as overly low, one's resources of emitted liking are clearly being wasted. On the other hand, when one sees someone overreciprocate, one is under some obligation to match the overreciprocation. If the other is reasonably well liked, and if the overreciprocation is not too great, the situation should not be particularly unsatisfying. One could seize (or hold in abeyance) the opportunity to invest more liking in this particular other, rendering the asymmetry merely temporary. It is less satisfying to be liked by someone whom one dislikes. The situation suggests that one will be hounded by unwanted invitations and will be at a loss to be both tactful and discouraging.

Since disliking is less common than liking, and since people probably avoid thinking about and interacting with those whom they dislike, it would be rather difficult to study quantitative asymmetry of disliking (that is, disliking reciprocated by a different degree of disliking). If the amount that one is disliked is held constant, then it would be expected—consistent with the rows of Table 1—that comfort would be greatest when emitted and received disliking match each other. However, if the level of emitted disliking is taken as given, then it would appear that—even for someone whom one dislikes a lot—it would be most comfortable if that person reciprocated the disliking qualitatively, but that nothing much would be gained by having the other person dislike one a lot rather than a little. One does not wish to receive unwanted positive invitations, but neither does one wish to be maligned or assaulted. The situation is rather complex and worthy of further study.

Potential embarrassment is probably another reason for feeling discomfort in liking, or being liked, too much. The tacit social agreement that people will help each other to save face (Goffman, 1955) includes due caution in approaching and withdrawing from others. In cases of asymmetric liking, not only is excess liking being "wasted," but also people may feel that it is wrong either to refuse a series of invitations on pretexts or to intrude where one is unwanted.

The data of Table 1 can be viewed from still other perspectives. An individual's experience with rewarding symmetric dyads and with unrewarding asymmetric ones could lead to relative autonomy of his reactions of discomfort to asymmetry. A person need not dwell on lost rewards in order to feel unhappy when he is deserted by a friend. The symmetry schema (DeSoto, 1960) is said to have the motivating property of a good figure in the gestalt sense.

On the other hand, the effects in Table 1 cannot be accounted for purely on the basis of asymmetry. One cannot, for example, interchange the corresponding columns and rows or substitute a mirror image for the table. It becomes necessary to consider the operation of parameters that may affect the force with which the balance model would be expected to operate in a given situation. In Table 1 it is only the fourth column, representing someone whom the subject likes particularly well, that spans nearly the entire available range of comfort—a finding in harmony with the suggestion that engagement or pertinence of a situation represents an important parameter (Price et al., 1966). A situation that logically manifests imbalance may in fact reveal "nonbalance" (manifest in indifference or ambivalence, as in the first two columns of Table 1, where differences are of rather small magnitude).

The results of Experiment II support the conclusions drawn from Experiment I. Since actual liking tends to be symmetric, those results in the first experiment which were attributed to asymmetry might have been due, at least partly, to the greater unreality or role playing inherent in the asymmetric cases. The instructions in the second experiment were designed to call up cases in which perceived asymmetry was actually salient.

The cases of "being liked in excess of emitted liking" fell along a continuum, most easily described by speaking of two categories. First are the cases, showing mild asymmetry, in which moderately high emitted liking was perceived as being reciprocated by liking at an even higher level (the bottom two rows of Table 3). In such cases, subjects' desire to be yet better liked, by a liked other, was stronger than their need to reduce mild asymmetry by being less liked. For these cases it should be noted that the (role-playing) shift in the perceived level of being liked would not have to be very great in *either* direction for the result to be asymmetric; thus, to be liked a given amount less would not necessarily yield gains in symmetry. Furthermore, the threat of discomfort at such reverse asymmetry should be great indeed, for one would lose the advantages of some of the reciprocated liking already achieved. Small wonder that the subjects in Experiment I felt comfortably secure at the prospect of their friends overreciprocating a bit!

The remaining category contains cases of marked asymmetry, in which emitted liking was well below the perceived level of being liked and was necessarily rather low in an absolute sense as well. In virtually all such cases, subjects showed greater comfort at being less liked than at being more liked. The magnitude of the effect could hardly have been greater, and the result is viewed as providing clear support for a dynamic symmetry schema: asymmetry is generally associated with discomfort. The expectation of symmetry of liking within the dyad is not solely motivated on an intellectual basis.

One moderate discrepancy deserves comment. In the first experiment nearly a third of the subjects *did* prefer to be well liked by a disliked other; whereas in the second experiment, confronted with actual marked asymmetry, no subject preferred to be even better liked by a disliked other. While this inconsistency between the two experiments could well be due to population, scaling, or other differences, one possible explanation is that discomfort is even more likely within asymmetric dyads than some naive role-playing subjects (governed by the common-sense view that being liked is routinely pleasurable) think it would be.

Previous studies, using either a learning task or a measure of projected discomfort, have shown the force of balance to operate within hypothetical or partly hypothetical social structures having at least three persons (e.g., DeSoto, Henley, & London, 1968; Morrissette, 1958; Price et al., 1965; Zajonc & Burnstein, 1965). In the present study, the first experiment demonstrates a strain toward symmetry within the dyad. The second experiment appears to mark the first time that such a finding has been verified for cases in which the cognitive imbalance actually developed in the real world and was not itself experimentally created. Both experiments indicate that the strain to reduce marked asymmetry is even greater than the general desire to be as well liked as possible.

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# LIKED AND DISLIKED PERSONS AS REINFORCING STIMULI[1]

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The hypothesis that liked persons can function as effective positive reinforcers and disliked persons as negative reinforcers was supported. One hundred ninth graders, working on a moderately difficult discrimination problem, were found to perform differentially as a function of their positive, neutral, or negative attraction to the peer whose photograph was presented contingent upon a particular response. Subjects performed in the following order, from best to poorest: those whose correct responses were reinforced with the printed word "right"; with the photo of a liked peer; with a blank card (while incorrect responses were followed by the photo of a disliked peer); with the photo of a neutral peer; and with the photo of a disliked peer. These findings were predicted from a theoretical framework in which interpersonal attitudes are defined in terms of the implicit anticipatory reward or punishment responses evoked by persons.

The authors have previously proposed (Lott & Lott, 1960, 1968) that interpersonal attraction can be meaningfully understood as attitudes toward persons, and that when positive attitudes are defined in S-R language as implicit anticipatory goal responses (Doob, 1947), a number of testable and theoretically consistent hypotheses may then be derived from Hull-Spence reinforcement theory. Derivations regarding the antecedents of attraction toward, or liking for, previously neutral persons have been tested; contiguity with reinforcement, either direct (James & Lott, 1964; Lott & Lott, 1960) or vicarious (Lott, Lott, & Matthews, in press), has been shown to be a sufficient condition for the development of liking, and persons associated with immediate reinforcement have been found to become better liked than those associated with delayed reinforcement (Lott, Aponte, Lott, & McGinley, in press). In each of these studies, as in a recently reported one by Griffitt (1968a), previously neutral persons were associated with reward simply by being present during another's goal attainment and not through any instrumental relation to it.

The present study deals not with the conditions necessary for the acquisition of liking but with the effects of liking on seemingly unrelated behavior. A liked person (or any liked object) is conceptualized as one who evokes implicit anticipatory goal responses ($r_g - s_g$) primarily as a result of previous association with reward, or through mediated or stimulus generalization. It follows, then, that liked persons can be classed as secondary reinforcing stimuli since such stimuli are said to derive their acquired reinforcement properties from their capacity to evoke fractional goal responses which have been conditioned to them (Hull, 1952).[3]

We have, as yet, done no direct empirical work on the acquisition of *negative* attitudes toward persons, but we have elsewhere suggested (Lott & Lott, 1968) that such attitudes may be usefully interpreted as equivalent to implicit anticipatory punishment or frustration responses as defined by Amsel (1958).[4] Such response-produced stimuli

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[3] Although it is difficult to conceive that liked persons could function as primary rather than secondary reinforcers, it is possible that this might be the case where liking for a person has resulted from conditions other than those specified above.

[4] Griffitt (1968b) directly manipulated anticipation of positive or negative reward from a stranger and found that attraction toward the individual from whom negative reinforcement was anticipated was significantly less than attraction toward the indi-

($r_p - s_p$ or $r_f - s_f$), conditioned to persons, should not only serve to elicit avoidance behavior but should also impart to such persons the power to act as negative reinforcers or punishers.

If liked persons can function as positive reinforcers and disliked persons as negative reinforcers then certain clear implications follow for the behavior of persons in the presence of those whom they like or dislike. Data gathered in a previous study (Lott & Lott, 1961) supported the hypothesis that degree of conformity to the opinions of fellow group members (in natural, existing groups) will be positively correlated with the degree to which these members like one another. This prediction was based on the assumption that the greater the cohesiveness of a small group (i.e., the greater the degree of intermember attraction) "the more should its members be able to function as secondary reinforcers for one another and, thus, the more should approval of a behavior (opinion) by members serve to mtaintain it and disapproval to extinguish it [p. 408]." The present study tests the reinforcement properties of liked and disliked persons in a more direct manner by presenting the photographs of liked, neutral, or disliked peers to individuals contingent upon their responses to a discrimination-learning task. Differential acquisition of the response designated as correct by the investigators is predicted to be a function of attraction to the person whose photo is presented as a reinforcer.

Byrne and his associates (Byrne, Young, & Griffitt, 1966; Golightly & Byrne, 1964) have found that college students could learn a discrimination problem if correct responses were followed by the presentation of attitude statements similar to their own and if incorrect responses were followed by attitude statements dissimilar to their own. The 1964 study employed three groups, one (R-P) which received conventional reward and punishment in the form of a "right" card and a "wrong" card, respectively, for correct and incorrect responses, one (N-N) which received statements equivalent to subjects' neutral attitudes for all responses, and one (S-D) for which correct responses were followed by an attitude statement printed on a card similar to the subjects' previously determined views while wrong responses were followed by attitude statements opposite or dissimilar from subjects' views. It was found that S-D, as well as R-P, subjects learned the problem while those in the N-N condition did not. The 1966 investigation replicated the Golightly and Byrne findings and also attempted to "determine the reinforcement properties of similar and dissimilar statements separately by pairing them with neutral statements." Positive results—that is, learning—were obtained only for the N-D group, and it was suggested that the neutral statements, too, had positive reinforcement properties, thereby retarding discrimination for subjects in the S-N condition. The investigators then paired similar, dissimilar, and neutral attitude statements with blank cards (assumed to be nonreinforcing stimuli). Under these conditions statistically reliable findings indicated that both similar and neutral statements can function as rewards (with some evidence supporting a greater effect on performance by the former than the latter), and that dissimilar statements are effective negative reinforcers.

The findings of Finley and Staats (1967) are also relevant to the present problem. Using sixth-grade children, they studied the relative effectiveness of words with positive, neutral, and negative evaluative meaning when used as reinforcing stimuli for a simple motor response. As predicted, the positive evaluative meaning words were found to strengthen the response while the negative words decreased its strength and neutral words had an intermediate effect.

The present experiment utilized the same discrimination-learning task as the Byrne studies, in a more extensive design, to test the reinforcing effectiveness of symbolic representations (photographs) of liked, neutral, and disliked peers on the performance of ninth-grade boys and girls. Five conditions were employed: R-B; L-B; N-B; D-B; and B-D. The letter on the left refers to the stimulus presented following a correct response while the letter on the right describes the stimulus presented following an incor-

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vidual from whom positive reinforcement was anticipated.

rect response. Thus, in each of the first four conditions, a blank card (B) followed an incorrect response while contingent upon a correct one was the word "right" printed on a card (R), or the photo of a liked (L), neutral (N), or disliked (D) peer, respectively. For the fifth group, the conditions of the fourth were reversed so that a photo of a disliked peer followed the incorrect response, and a blank card the correct one.

It was predicted that the experimental groups would differ in the percentage of correct responses made by subjects over trials in the following order from best to poorest performance: R-B, L-B, B-D, N-D, and D-B. Superior performance was expected of the group receiving conventional reinforcement, that is, the word "right," followed by the group reinforced by the picture of a highly liked peer. B-D subjects were also expected to show an increment in performance, but since the photo of a disliked person represented a "something" as compared with the "nothing" of a blank photo, these subjects for whom correct responses were followed by the blank card were expected to learn less well than those in the R-B or L-B groups. Subjects reinforced by the photo of a neutral peer were expected not to perform as well as those in the previously mentioned groups since neutral peers should have only minimal reinforcement properties, positive or negative. Finally, it was predicted that the group receiving a photo of a disliked person following a correct response (D-B) would not learn the correct response or that the probability of the incorrect response would increase.

## METHOD

### *Subjects*

Fifty boys and 50 girls were selected from seven ninth-grade homeroom classes in a public junior high school. The selection criteria will be described subsequently.

### *Procedure*

All members of the seven ninth-grade classes were given the same 30-point liking scales on which they were instructed to rate each of their same-sex classmates. Each scale consisted of 15 descending steps on the left, underneath which was printed the word "like," and 15 ascending steps on the right, underneath which appeared the word "dislike." Each student received a booklet containing a scale for each same-sex classmate; the ratee's name was printed on the top of the page. The booklets were distributed during a regular homeroom period and the class was instructed as follows:

> Underneath the name [of the person to be rated] is . . . a rating scale, which looks . . . like steps going down and steps going up. These steps represent different degrees of liking and disliking. I want you to tell me how you feel about the boy or girl whose name is at the top of the page by marking one of the steps with an X. If you like him [or her] very, very much, mark one of the big steps at he left. If you like him just a little bit, then you should mark one of the smaller steps. If you are neutral, i.e., . . . do not like the person but do not dislike him . . . then you should mark one of the small steps in the middle of the scale. If you dislike the person somewhat, then mark one of the smaller steps on the right side of the scale, the dislike end, and if you do not like the person very, very much then you should mark one of the big steps at the right. Remember, you are to show *just how much* you like or do not like the person . . . mark only one step on each page.

Each page in the booklet contained, in addition to a liking scale, the following two questions dealing with the respondent's familiarity with the ratee: How often do you see this person in school? (to be answered by checking often, sometimes, or seldom); How often do you see this person outside of school? (to be answered by checking often, sometimes, seldom, or never). The students were asked to circle their own names when they came to their page and were assured that no school official or classmate would see their booklets.

The rating scale data were utilized to select subjects for the discrimination-learning task, 10 of each sex for each of the following conditions: right-blank (R-B); like-blank (L-B); neutral-blank (N-B); dislike-blank (D-B); and blank-dislike (B-D). For subjects in conditions L-B, N-B, and D-B the photo of a highly liked, neutral, or disliked same-sex peer was required for presentation to the subject following a correct response; for B-D subjects the photo of a disliked peer was needed for presentation following an incorrect response; only the subjects in the R-B condition required no reinforcer photos. A student was placed in one of these five conditions as his rating scale booklet revealed a strong *like* or strong *dislike,* or a *neutral* rating for a classmate with whom he was *moderately familiar,* that is, whom he saw often or sometimes in school but seldom or never outside of school. The intent in placing these familiarity restrictions on the relation between a subject and his potentially reinforcing stimulus person was to eliminate, as far as possible, the influence of differences in familiarity on the reinforcing effectiveness of the photos.[5]

Students whose responses to the rating scales permitted their assignment to more than one condition were placed randomly. All students were eligible for Condition R-B and assignment to it was random. The liking ratings of peers selected as reinforcing stimuli for subjects in Group L-B were either 15 or 14 ($M = 14.8$); the ratings of peers selected as reinforcing stimuli for subjects in Group N-B ranged from 0 through 3 ($M = 1.2$); and the ratings of peers selected as reinforcers for D-B subjects ranged from −7 through −15 ($M = -12.4$); and for B-D subjects from −3 through −15 ($M = -11$). Extreme dislike ratings were less frequent than extreme like ratings and, in general, were less frequently made by girls than by boys.

After selection of the 100 subjects, photographs were taken of those particular boys and girls who were to serve as potential reinforcers and a large number of additional ninth graders. The photographs were all taken by a professional photographer in a standardized manner within a mobile research unit in the school parking lot.

The subjects were divided into two groups of 50, each containing 5 boys and 5 girls within each of the five experimental conditions. Each group of 50 was assigned to one of two different experimenters (Experimenter 1 and Experimenter 2). Within each group the order in which subjects were called for the learning task was predetermined on a purely random basis. Typically, nine subjects were run each day with each experimental session lasting approximately 20–30 minutes.

The learning task, a visual discrimination problem, was taken from Byrne (Byrne, Young, & Griffitt, 1966; Golightly & Byrne, 1964). Eight stimulus cards each contained two figures in one of eight possible combinations of shape (circle or square), size (large or small), color (black or white), and position (right or left). Subjects were given a maximum of 96 trials; that is, the set of eight cards was presented 12 times. The order of cards within each set was random. Subjects who gave the same size response 100% of the time on 16 consecutive trials were judged to have learned the discrimination. Choice of the large stimulus was positively reinforced for one-half of the subjects in each condition while choice of the small stimulus was considered correct for the other half.

Subjects in all five experimental conditions were treated identically except with respect to the reinforcing stimuli presented to them, as previously described. A "blind" procedure was followed by each experimenter with respect to subjects in the L-B, N-B, and D-B conditions. The reinforcing photos for these subjects were attached to their data sheets and no notation of the subject's experimental condition was visible; the experimenter simply presented a photograph, with no knowledge of its valence for the subject, following the responses which had been previously designated to be "correct" for that subject. The R-B condition, requiring a "right" card following a correct response, and the B-D condition, requiring a photo following an incorrect response, could not be run blind. The same photograph was used as a reinforcer throughout the series of trials for a given subject.

Before being instructed in the discrimination task, each subject was asked if any of his classmates had spoken to him about the procedure which was to follow. No one admitted more than vague prior knowledge. Instruction to the subject was then given, as follows:

> This is a learning experiment. . . . I have here a stack of cards which you are going to look at. Each card will have a circle and a square on it. You will be shown a number of such cards, one at a time. Sometimes the circle will be small, sometimes large; sometimes white, sometimes black; sometimes it will be on the right and sometimes on the left. Also, the square will be small or large, white or black, on the right or on the left. Each time I show you a card, you are to choose either the circle or the square. Say your choice out loud by describing its size, color, and shape, for example, "Large white square." . . . After you have made your choice I will put something here in front of you on the table. Then I will take it away . . . and I will show you a new card. . . .

It should be noted that subjects were *not* instructed to try to discover a correct response nor given any kind of problem-solving set. There was no suggestion that one response would be preferred over another.

The subject was asked, the first time he was presented with a photograph, "Do you know who this is?" Following the learning trials, each subject was interviewed briefly regarding his understanding of the response contingencies. The experimenter also thanked the subject and urged him not to discuss his experience with any of the other students. It was explained that the students were being tested under different conditions and that no one could be helped by prior information.

## RESULTS

### *Performance on the Discrimination Problem*

To analyze the learning data, the trials were divided into six blocks of 16 trials each and the subject's score on each block was his percentage of correct responses. A 2 × 2 × 5 × 6 analysis of variance for repeated measures was performed on these scores in order to evaluate the influence of the experimenters, sex of the subject, the experimental conditions, and the blocks of trials. This analysis is summarized in Table 1 and the acquisition

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[5] Nine slight exceptions had to be made with regard to the 4-point familiarity criteria in order to get sufficient numbers of subjects for the conditions.

TABLE 1

SUMMARY OF ANALYSIS OF VARIANCE OF PERCENTAGE OF CORRECT SCORES

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Between | | | |
| Experimenter (E) | 1 | 6,056.31 | 4.58* |
| Sex (S) | 1 | 3,534.44 | 2.67 |
| Experimental Conditions (C) | 4 | 12,082.36 | 9.13*** |
| E × S | 1 | 89.13 | <1 |
| E × C | 4 | 5,120.44 | 3.87** |
| S × C | 4 | 1,157.48 | <1 |
| E × S × C | 4 | 1,719.01 | 1.30 |
| *S*s within groups (error) | 80 | 1,323.18 | |
| Total | 99 | | |
| Within | | | |
| Blocks of Trials (T) | 5 | 931.78 | 5.72*** |
| E × T | 5 | 281.78 | 1.73 |
| S × T | 5 | 129.91 | <1 |
| C × T | 20 | 484.19 | 2.98*** |
| E × C × T | 20 | 118.76 | <1 |
| E × S × T | 5 | 120.53 | <1 |
| S × C × T | 20 | 174.64 | 1.07 |
| E × S × C × T | 20 | 285.77 | 1.76* |
| T × *S*s within groups (error) | 400 | 162.82 | |
| Total | 500 | | |

* $p < .05$.
** $p < .01$.
*** $p < .001$.

curves for the five experimental groups are presented in Figure 1.

A highly significant conditions effect indicates that, as predicted, performance of subjects on the discrimination task, over all the trials, differed as a function of the nature of the reinforcing stimuli presented to them. Figure 1 shows clearly that the subjects whose correct responses were reinforced by the word "right" (R-B) performed best, followed by those whose correct responses were reinforced by the photo of a liked peer (L-B). Both the subjects reinforced with a neutral peer photo (N-B) and those who received a blank card following correct responses and a disliked peer photo after incorrect ones (B-D) increased their percentage of correct responses, but only slightly, and remained close to the 50% correct level. The subjects whose correct responses were followed by the photo of a disliked peer (D-B) performed most poorly on the task; in fact their percentage of correct responses decreased significantly over trials ($t = 1.88$, $p < .05$, one-tailed, difference between first two and last two trial blocks). The significant within-groups trials effect shows that performance changed reliably over trials and, as predicted, there is a significant interaction between trials and conditions, indicating differential learning associated with the experimental treatments.

The between-groups experimenter variable proved to be significant, an unanticipated finding, as did the interaction among all four variables, experimenter, sex, conditions, and trials. Cell-by-cell comparison of 120 cells (two sexes in each of five conditions, under each of two experimenters, across six blocks of trials) was rejected in view of the small *n* per cell (5), the lack of a significant overall sex difference, and the very large variance contributed to the total by the experimental conditions. To get an indication of experimenter differences, and between-condition differences associated with each experimenter, the mean percentage correct for Trial Blocks 5 and 6 (the final third of the learning trials) was calculated for each group of subjects separately by experimenter and condition and *t* tests were performed. These data are given in Table 2.

It can be seen that the performance of

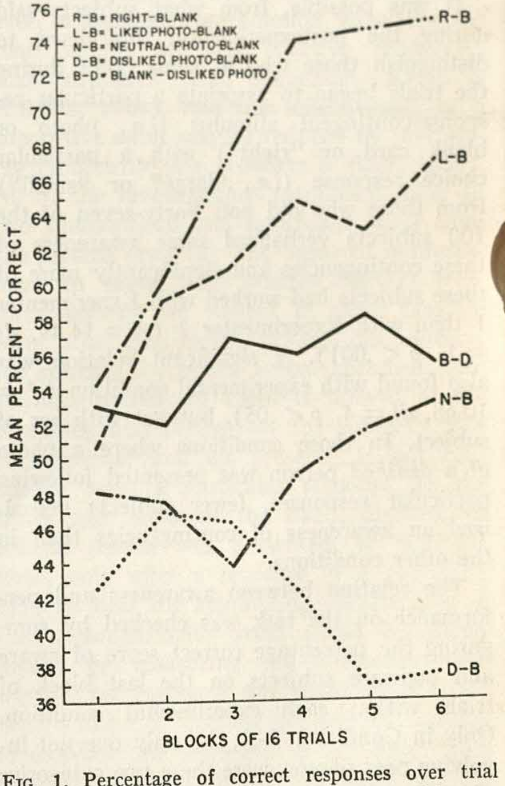

FIG. 1. Percentage of correct responses over trial blocks as a function of experimental conditions.

Experimenter 1 subjects was superior to that of Experimenter 2 subjects under all conditions except D-B, but comparisons of mean scores yielded a statistically reliable difference only for the R-B condition.

With respect to differences between conditions for subjects working with Experimenter 1, the R-B group is significantly better than each of the other groups, the L-B group is better than the B-D, N-B, and D-B groups, but only the difference with the latter is statistically reliable, and both the B-D and N-B groups differ significantly from the D-B but not from each other, although their means are in the expected direction. For subjects working with Experimenter 2, the L-B and R-B means do not differ reliably, but each of these groups performed significantly better than the D-B group. It is of interest to note the variability within conditions; greatest variability occurred within the N-B group, regardless of experimenter.

### *Awareness of Contingencies*

It was possible, from what subjects said during the postexperimental interviews, to distinguish those who at some point during the trials began to associate a particular response-contingent stimulus (i.e., photo or blank card or "right") with a particular choice response (i.e., "large" or "small") from those who did not. Forty-seven of the 100 subjects verbalized some awareness of these contingencies and significantly more of these subjects had worked with Experimenter 1 than with Experimenter 2 ($\chi^2 = 14.49$, $df = 1$, $p < .001$). A significant relation was also found with experimental condition ($\chi^2 = 10.68$, $df = 4$, $p < .05$), but not with sex of subject. In those conditions where a photo of a *disliked* person was presented following particular responses, fewer subjects verbalized an awareness of contingencies than in the other conditions.

The relation between awareness and performance on the task was checked by comparing the percentage correct score of aware and unaware subjects on the last block of trials within each experimental condition. Only in Condition R-B, the only one not involving peer photos, were these two categories of subject found to differ reliably ($p = .002$, Mann-Whitney *U* test, two-tailed), with better performance by the aware subjects.

TABLE 2

MEAN PERCENTAGE CORRECT IN TRIAL BLOCKS 5 AND 6 FOR GROUPS UNDER DIFFERENT REINFORCEMENT CONDITIONS WORKING WITH DIFFERENT EXPERIMENTERS

| Condition | $E_1$ | $E_2$ |
|---|---|---|
| R-B | | |
| *M* | $91.56_{abc}$ | $58.44_a$ |
| *s* | 18.1 | 17.1 |
| L-B | | |
| *M* | $68.75_a$ | $61.56_b$ |
| *s* | 20.7 | 25.5 |
| B-D | | |
| *M* | $63.75_b$ | 49.69 |
| *s* | 21.5 | 11.3 |
| N-B | | |
| *M* | $54.69_c$ | 50.00 |
| *s* | 29.1 | 26.7 |
| D-B | | |
| *M* | $32.50_{abc}$ | $41.88_{ab}$ |
| *s* | 25.7 | 18.9 |

Note.—Means with subscripts in common are significantly different, that is, $p$ at least $<.05$, one-tailed ($t$ tests). Mean comparisons are for reinforcement condition groups run by the same experimenter.

## DISCUSSION

Our data can be said to support the hypothesis that a liked person (symbolically represented in this study by his photograph) may effectively function as a positive reinforcer; presentation of such a stimulus following a particular response on a discrimination task was found to increase the probability of that response to a greater extent than presentation of the photograph of a neutral or a disliked person. The photograph of a disliked person proved to be an effective negative reinforcer: subjects whose correct responses were followed by such a stimulus significantly decreased their number of such responses and increased their number of incorrect ones, performing more poorly than subjects whose correct responses were followed by photos of neutral persons or liked persons; subjects whose incorrect responses were followed by a photo of a disliked peer and correct responses by a blank card increased the latter but not greatly. Why the negatively reinforcing effect of disliked peer photos was stronger under the D-B than under the B-D condition is not immediately apparent.

The experimental conditions, trial blocks, and their interaction were found to be significant sources of variation, as predicted. Overall, subjects performed in the following order, from best to poorest: those whose correct responses were reinforced with "right"; with the photo of a liked peer; with a blank card (while incorrect responses were followed by the photo of a disliked peer); with the photo of a neutral peer; and with the photo of a disliked peer. Under each experimenter, considered separately, the liked photo condition (L-B) was reliably superior to the disliked photo condition (D-B). Within the Experimenter 1 group, the B-D and N-B conditions were also significantly better than the disliked photo condition (D-B), but within the Experimenter 2 group these differences were not statistically significant, although subjects in the former two groups performed better, as expected, than those in the latter.

Variability within the neutral peer group was greatest, under both experimenters, thus reducing the chances of obtaining significant differences between it and other groups. Under both experimenters, however, subjects in the neutral condition performed more poorly than those in the liked photo, and better than those in the disliked photo, groups. The finding of greater variability among the neutral photo subjects is quite reasonable since, if attitudes toward classmates had changed between the time the initial liking ratings were obtained and the time the learning task was presented, it is more probable that the neutrally rated peers shifted in their attractiveness in either a positive or negative direction than that highly liked or highly disliked peers changed in their attractiveness. It is likely, therefore, that some subjects in the neutral photo condition were actually being reinforced with the photos of liked or disliked peers, which could explain their more variable performance on the discrimination task.

Despite controls for experimenter bias, an unanticipated experimenter effect indicated that performance was generally better under one experimenter. This may be attributable to the reaction of subjects to certain personal characteristics of the two experimenters. Experimenter 1, who was older and more experienced, may have been more effective in putting subjects at ease and involving them in the task, whereas Experimenter 2, who looked more like a peer and not like a typical adult teacher figure, may have been more threatening and hence performance-disrupting. Congruent with the experimenter effect is the finding that among those subjects who verbalized, at the end of the task, the response-reinforcement contingencies, significantly more were from the Experimenter 1 group.

### *Other Studies of Persons as Reinforcers*

A number of recent studies have been concerned with the relation between performance on tasks and the performer's attitude toward the experimenter or dispenser of reinforcements. Some investigations have found that performance is better when the reinforcing agent is presumed, or known, to be liked by the subject as a result of prior positive interaction or instructions for "compatability" or other reasons (e.g., McCoy & Zigler, 1965; Sapolsky, 1960). Other studies, however, in apparent contradiction, have reported that parents are less effective reinforcers than strangers (e.g., Stevenson, Keen, & Knights, 1963), and that effectiveness of a supportive adult may be reduced by pretraining familiarity (e.g., Gewirtz & Baer, 1958). All of the investigations mentioned differ in two fundamental and significant ways from the present one: (*a*) The agent of reinforcement in these other studies delivers reinforcement to the subject in some tangible form, other than simply through his presence, for example, by giving verbal approval; and (*b*) the reinforcing agent is present at the beginning and *throughout* the subject's task performance, and his presence is not contingent upon a particular response. In the study here reported the liked, disliked, or neutral person was presented, in symbolic form, only *after* a response of a particular kind so that the reinforcing effectiveness of the person-stimulus itself could be ascertained by measuring the frequency with which that response was made over trials. Where liked or disliked persons are present throughout the sequence of behavior, their possible (and probable) influence on the drive level of the performer introduces an additional variable (Lott & Lott, 1966, 1968; Matthews, 1968).

Hartup (1967) has reported that when verbal approval was administered to nursery school children by liked or disliked peers, performance on a Marble-in-the-Hole Game was better in the group reinforced by disliked peers. His use of an experimental model, however, in which peers function as experimenters, is characterized by the same two problems raised above. Under conditions where the disliked peer is present throughout task performance, it is possible that he can raise the performer's drive level (by evoking anxiety) more than a liked peer, thus producing better performance on a simple task. The findings of Stevenson et al. (1963) and others (e.g., Aronson & Linder, 1965) have also suggested that approval which is unexpected (as from a disliked person or a stranger) may have greater reward potential than that which is expected (as from a friend or parent).

Closer in design, and therefore more relevant to the present investigation, is a study by Horowitz (1962) in which preschool children pulled levers and were able to view, after every third pull, the photo of a best friend, a neutral peer, or a blue light. On a measure of total time, in minutes, that subjects spent on the task before indicating that they were finished, significant differences were found between the "light" and "best friend" group and between the latter and the "neutral" group, with the most time spent on the task by subjects who were reinforced by a picture of their "best friend" and the least amount of time by those reinforced by the light. These findings held for the very young children (mean age: 3 years, 10 months) but not for the older ones (mean age: 5 years, 1 month), possibly because the task was too simple and too dull for the latter.

In the present investigation, ninth-grade students, working on a moderately difficult discrimination problem, were found to perform differentially as a function of their positive or negative attraction to the peer whose photograph was presented contingent upon a particular response. It seems reasonable to conclude that the obtained data support the general proposition tested by the experiment, that liked persons can function as effective positive reinforcers while disliked persons can produce the same effects as negative reinforcers. This prediction is based on a theoretical position in which interpersonal attraction is defined in terms of implicit anticipatory reward or punishment responses, evoked by persons, depending upon the nature of one's direct past experience with the person (or as a result of stimulus or mediated generalization). While the empirical findings of this investigation do not by themselves validate the theory, they do support an hypothesis derived from it and together with other supportive findings tend to suggest that the theory has merit. To say that subjects in the present experiment generally behaved in such a way as to maximize their opportunities to view the photo of a liked person and to minimize their chances of viewing a disliked peer would be adequately descriptive but would have little logical and theoretical relationship to a variety of other findings regarding the antecedents and effects of interpersonal attraction.

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# EFFECTS OF CONCEPTUAL STRUCTURE, FAILURE, AND SUCCESS ON ATTRIBUTION OF CAUSALITY AND INTERPERSONAL ATTITUDES [1]

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Thirty-two subjects of simple and 32 subjects of complex conceptual structure participated as decision-making dyads in an experimental simulation. The dyads were exposed to increasing failure or increasing success. The effect of failure and success on subjects' estimates of causality for their "current situation" and its effect on interpersonal attitude within the group were investigated. It was found that subjects take increasingly more credit for success as success increases, but do not take similar credit for increasing failure. It was found that attitudes toward other group members become more favorable when the group is credited with success. Attitudes remain constant when causality attributions show constancy (failure). The effect of success and failure on attribution of causality, and with it the effect on attitudes, was more pronounced for simple than for complex subjects.

Considerable research has shown that success on group tasks results in increased attraction of group members toward each other (e.g., Berkowitz & Levy, 1956; Deutsch, 1959; Gilchrist, 1952). Other research has indicated that real, threatened, or anticipated failure can also lead to attraction among subjects (Berkowitz, Levy, & Harvey, 1957; Lanzetta, 1955; Thibaut, 1950; Wilson & Miller, 1961). If one considers success and failure as opposite ends of a single dimension, and if one conceives of attitudes toward fellow group members as a linear function of the dimension, then the aforementioned results seem paradoxical. However, research by the present authors has shown that success and failure (as perceived by subjects) tend to be independent as long as the environment in which success and failure is experienced is sufficiently complex. The fact that Lerner (1965) has demonstrated that failure in task-oriented groups can lead to more unfavorable attitudes suggests that additional research is necessary to determine more closely the relationships between success or failure and attitudes.

In his research on the effect of group failure on attitudes, Lerner (1965) was able to suggest a variable which may mediate the success/failure–attitude relationship. The responsibility for failure was experimentally controlled in his research. To obtain the perception of a dyad partner as responsible, the experimenter informed his subjects that failure of their group (to win money) was due to the poor performance of the partner. Lerner found that the partner was less valued after failure. Since Lerner's results differ from those of other researchers (Berkowitz et al., 1957; Lanzetta, 1955; Thibaut, 1950; Wilson & Miller, 1961), the *cause* of success or failure as perceived by a participating subject may be an important variable. Differential attribution of causality may well produce different interpersonal attitudes. For instance, one might suggest that (*a*) placing the blame for failure on some external condition, (*b*) placing the blame with oneself, and (*c*) placing the blame with one's partner(s) would all have different effects on interpersonal attitudes after failure. Similar differences would be likely for attitudes growing out of success experience. Further, it is possible that external assignment of blame (for instance, by an authority, e.g., the experimenter) could result in unfavorable attitudes toward the blamed person, while unsupported (e.g., self-placed) blame for failure would not produce as unfavorable attitudes. Similarly, discrep-

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ant effects might be observed for externally placed credit for group success as compared with self-placed credit. Some of such questions are investigated in the present paper.

In complex social interactions, and in simulations which are designed to parallel such interactions, it is often difficult for subjects to know who has caused the level of success or failure they experience. Under such conditions, where would the blame for failure or the credit for success be placed?

Clinical and motivation research (e.g., Eysenck, 1964) suggests that in ambiguous situations most people would blame their failure on factors outside the group, while credit for success would be taken by the group members themselves, either individually or jointly. If this is true, then increasing the success of a task-oriented group should result in the placing of more and more of the credit for success within the group. Since group members would likely be seen as partially responsible, interpersonal attitudes toward the group members might become more favorable. Such findings would be in agreement with the research reported by Berkowitz and Levy (1956), Deutsch (1959), and Gilchrist (1952).

Expectations for failure conditions should be quite different. If the previous reasoning is followed (Eysenck, 1964), the group members in ambiguous situations should be expected to see the causal agent for failure as something outside the group. Since group members would not have contributed to failure, one would *not* expect attitude shifts toward a more unfavorable direction with increasing failure rates. Rather, one would expect either no attitude change or (if experimental conditions are conducive) attitude change in a more favorable direction. The results of more favorable attitudes after failure, reported by Berkowitz et al. (1957), Lanzetta (1955), Thibaut (1950), and Wilson and Miller (1961), might be explained as an effect of the time the members spent together working jointly on a task where group members experience joint discomfort (where misery loves miserable company), or similar variables. In the case of findings of more unfavorable attitudes after failure (Lerner, 1965), causality might be attributed to the decisions of the devalued group members.

Another variable that may affect the attitudes arising out of success and failure experience is the simplicity versus the complexity of subjects' conceptual structure. Crano and Schroder (1967) and Streufert (1966) have shown that the attitudes formed and maintained (or changed) by groups selected for differences on this personality variable are consistently different. Such results were predicted on the basis of the expected differences in attitudinal dimensionality proposed by complexity theory (Driver & Streufert, 1966; Schroder, Driver, & Streufert, 1967; Streufert & Driver, 1967). According to complexity theory, attitudes of simple subjects should be primarily based on unidimensional interpersonal perceptions, while complex subjects should perceive others in a more multidimensional fashion.[3] Such differences in dimensionality have been demonstrated by researchers employing a number of experimental methods (e.g., Karlins, 1967; Schroder et al., 1967; Sieber & Lanzetta, 1964; Streufert, 1966; Streufert & Driver, 1965; Streufert & Schroder, 1965). As a consequence of differences in perceptual dimensionality in subjects, one may expect differences in subjects' information search and information utilization (as found by Streufert, Suedfeld, & Driver, 1965), which, in turn, should produce divergent attitudes that are formed on the basis of information content and perceptual dimensionality (cf. Crano & Schroder, 1967; Streufert, 1966). More specifically applied to the present experimental setting, attitude formation and attitude change should proceed as follows:

Generally, all subjects, no matter whether simple or complex, tend to form initially favorable attitudes toward other group members when placed in a group which is competitively interacting with another group (cf. Sherif, Harvey, White, Hood, & Sherif, 1961). Relevant information about the performance of the group and its members, which is presented *after* the initial attitude has been formed, may have evaluative implications for the group and for its members. Failure information, suggesting that the group members are incompetent or "bad" in the task at hand, should be inconsistent with the established view the group members have of each other as "good." In contrast, success information should be consistent with established attitudes. How will the group and the group members deal with failure and success information? If the group is experiencing failure, then the evaluation implied in the failure message is quite discrepant from the attitudinal evaluation that the group members had for each other. Can a subject consider another group member (or, for that matter, himself) to be both "good" and "incompetent" at the same time? If the two positions were related on a single evaluative dimension, then one should expect a shift in the attitude toward a less favorable position. Such a result was obtained by Lerner (1965) when an authority stated that the partner was to blame for the "incompetent" decision making leading to failure. However, in most research an authority-based blame of the other group member(s) is not given. In the absence of such an "easy" solution, failure has often led to favorable attitudes, frequently explained as the effect of the "joint misery" of the group members. Apparently such subjects did not use the failure information to modify their attitudes. Since the failure was a joint one, derogating another group member because of his "incompetence" could have implications for the subject's attitudes toward himself, making an attitude change less likely. The simplest solution to the dilemma is a refusal to accept the group's responsibility for the failure, so that a reevaluation of the group members in terms of competence is avoided. Such a solution then permits the maintenance of (or increase in) favorable attitudes.

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[3] Correlations between simplicity–complexity of conceptual structure with intelligence and other motivational, cognitive, and personality measures tend to be low and insignificant (see, e.g., Schroder et al., 1967; Vannoy, 1965).

Such arguments should apply to groups consisting of simple subjects under failure, in other words, to failing subjects whose perceptions are primarily unidimensional. For the complex subject, who should be able to invoke additional dimensions, there should be less difficulty in coping with the failure information and with its relationship to established attitudes toward other group members. "Liking" and "failure" can be integrated through the use of another differentiated (more or less orthogonal) judgment[4]; for instance, the complex group member might conclude that the group is insufficiently familiar with the skills required. He may assume that the opposing group probably has greater skills from past experience. However, he may conclude that he and his group are good players in any case, relative to their knowledge, and that after they learn more, they probably will do better.[5] In other words, there is no necessity for complex subjects to see the responsibility for failure as primarily external to the group's competence. Such an integration of the evaluative and an orthogonal "experience" dimension should not be as possible for simple subjects (no matter how intelligent).

What, however, would happen if the group finds it has been successful? Let us again assume that the group members have initially favorable attitudes toward each other. Success information, indicating that the "good" group members are also "good" in terms of competence, is consistent, and no integration is necessary to relate the two concepts. Both simple and complex subjects should respond by attributing more causality to decisions made by their own team and by forming and maintaining favorable attitudes. However, the complex person, who tends to be more open to additional facets in his environment (cf. Streufert & Driver, 1965), should not perceive extremely high levels of success as caused by one component of the situation *alone*. In other words, one might expect that the attribution estimates of complex subjects would level off under high success, while the estimates of simple subjects should continue to increase. Parallel findings should be predicted for subjects' attitudes toward other group members.

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[4] The predictions derived on the basis of complexity theory suggest that impressions of others formed on the basis of particular information are not likely to be modified by simple subjects when new and inconsistent information becomes available. Complex subjects should have no difficulty in integrating the two sets of information. Such predictions have been supported by previous research (Streufert & Driver, 1967; Streufert & Schroder, 1963).

[5] This is the conclusion that is actually reached by complex subjects most frequently. Simple subjects, on the other hand, generally refuse to accept that failure has anything to do with their own performance.

## METHOD

### *Subject Selection*

Five hundred and ninety-six students in introductory psychology classes at an eastern state university were given a series of personality tests in class administration. Subjects were paid $1 for participation. Among the tests given was the Sentence Completion Test (SCT—Schroder et al., 1967; Schroder & Streufert, 1963). The SCT has been repeatedly validated as a measure of the simplicity–complexity of conceptual structure (e.g., Karlins, 1967; Stager, 1967; Streufert, 1966; Streufert & Driver, 1965; Streufert & Schroder, 1965; Streufert, Suedfeld, & Driver, 1965). The SCT was used to distinguish between subjects who are complex (scores in the integration range—10 through 16) and subjects who are simple (scores below the differentiation range—1 and 2). From the available subject pool, 32 complex and 32 simple subjects were selected at random. One-half of the complex and one-half of the simple subjects ($n = 16$ each) were placed into the success condition. The other halves were placed into the failure condition.

### *Task*

The subjects were placed as pairs (homogeneous in simplicity or complexity of conceptual structure) into dyad decision-making teams. They were instructed to act as equal rank commanders with responsibility for military, economic, and intelligence functions of an experimental simulation. The task, a simulated internation decision-making situation, is described by Streufert, Kliger, Castore, and Driver (1967) and has been used in a somewhat simpler form (Streufert, Clardy, Driver, Karlins, Schroder, & Suedfeld, 1965) in considerable previous research (e.g., Stager, 1967; Streufert & Driver, 1965; Streufert, Driver, & Haun, 1967; Streufert & Schroder, 1965; Streufert, Suedfeld, & Driver, 1965; Suedfeld & Streufert, 1966). The teams were told that they were playing a tactical and economic game against another team and that the experimenters would serve as judges (assisted by a computer). Subjects were free to make any kind and number of decisions possible within the constraints of available resources. They were told that the experimenters would determine "wins" and "losses" based on the decisions made by both teams. Such "consequences of decisions made by subjects" were fed back to the subjects.

In fact, subjects were playing the game against a predetermined program.[6] All information fed back to the subjects was constant across all success or failure runs, so that all teams in the same condition received the same messages, no matter what their specific decisions had been.

All dyads participated in the game for seven consecutive 30-minute periods. To avoid an end effect, teams were not told which period would be the last. During each period the teams received seven typed messages, in equally spaced time intervals. Two messages contained information about military, two messages about negotiation, two about economic, and one about intelligence components of the game. The order of messages for each period was randomized. During the first period, one message reported failure (for subjects in the failure condition) or success (for subjects in the success condition). The remaining six messages were neutral in content. During the second period, two messages reported failure or success, and so forth, until in the seventh period all seven messages reported failure or success. The order of failure or success messages versus neutral messages was randomized.

### *Data Collection*

Each of the half-hour playing periods was followed immediately by an intermission, used to fill out a number of scales. Among such was the following: "If you consider the situation as it has developed during the last period, you might come to some conclusions about causal factors in the development of the current situation. Please estimate what percentage of the current situation you feel is due to each of the following: (*a*) decisions made by your team; (*b*) decisions made by the opposing team; (*c*) various chance factors; (*d*) arbitrary decisions made by the experimenters; and (*e*) characteristics of the environment; the total should be 100%." Percentage estimates, as modified,[7] were used as raw data in the attribution of causality analysis.

Also, subjects were asked to rate their partner on six semantic differential scales that were presented. Among such were two evaluative scales which had been found to correlate most highly with the good–bad scale and intercorrelate most highly with each other in previous research by Streufert (1965, 1966). The scales used were "bright–dark" and "awful–nice." The sums of the two scales, with a low score indicative of highly favorable evaluation, were used as raw data for the attitude analysis.

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[6] For descriptions and manuals of the game (TNG), see Streufert, Castore, and Kliger, 1967; Streufert, Clardy, Driver, Karlins, Schroder, and Suedfeld, 1965; Streufert, Kliger, Castore, and Driver, 1967.

[7] In the present data, we are less interested in the attribution categories for "chance factors," "arbitrary decisions," and "characteristics of the environment," since such are external to the intent of the experiment and operate as checks on the experimental manipulations only. For this reason, the responses of each subject in the categories are combined into a total score. The combination of categories is justified by the finding that subjects' estimate for "decisions made by the opposing team" and "decisions made by your team" accounted for more than 80% of the cause estimates (except in the first period). Further, the estimates for the three categories which were combined did not differ from each other in level or pattern over playing periods. Their joint level as a "percent of situation due to other causes" category can then be viewed as a check on a possible "manipulation error" in the experiment.

## RESULTS AND DISCUSSION

### *Analysis 1: Attribution of Causality*[8]

As stated previously, raw data for attribution of causality were obtained as percentage estimates[9] adding to a total of 100%. To correct for variance distortions (see Footnote 9)the raw data were transformed to arcsin values. A separate three-way mixed-design (two between, one within) analysis of variance was performed on each of the three sets of data. The results of the analysis are presented in Table 1. A visual presentation, with periods of play plotted on the abscissa (in-

TABLE 1

ANALYSIS OF VARIANCE FOR CAUSAL FACTORS
(ARCSIN VALUES OF PERCENTAGES)

| Source | *SS* | *MS* | *df* | *F* |
|---|---|---|---|---|
| Analysis for attributions to decisions of *S*'s own team | | | | |
| *M* | 934.4716 | 934.4716 | | |
| Success-Failure (A) | 7.6390 | 7.6390 | 1 | 16.19** |
| Simple-Complex (B) | 2.4818 | 2.4818 | 1 | 5.26* |
| Periods (C) | 20.0483 | 3.3414 | 6 | 28.06** |
| A × B | .5026 | .5026 | 1 | 1.06 |
| A × C | 6.9607 | 1.1601 | 6 | 9.74** |
| B × C | 2.9920 | .4987 | 6 | 4.19** |
| A × B × C | .6854 | .1142 | 6 | <1.0 |
| *S*s (A × B) | 28.3084 | .4718 | 60 | |
| C × *S*s (A × B) | 42.8829 | .1191 | 360 | |
| Analysis for attributions to decisions of opposing team | | | | |
| *M* | 381.8532 | 381.8532 | | |
| Success-Failure (A) | 8.3829 | 8.3829 | 1 | 9.81** |
| Simple-Complex (B) | 5.7256 | 5.7256 | 1 | 6.73* |
| Periods (C) | 22.5101 | 3.7517 | 6 | 23.22** |
| A × B | .2728 | .2728 | 1 | <1.0 |
| A × C | 2.0687 | .3448 | 6 | 2.13* |
| B × C | 3.2499 | .5416 | 6 | 3.35** |
| A × B × C | 1.9971 | .3329 | 6 | 2.06 |
| *S*s (A × B) | 51.2862 | .8548 | 60 | |
| C × *S*s (A × B) | 58.1642 | .1616 | 360 | |
| Analysis for attributions to other causes | | | | |
| *M* | 587.7235 | 587.7235 | | |
| Success-Failure (A) | 29.4265 | 29.4265 | 1 | 59.72** |
| Simple-Complex (B) | .6066 | .6066 | 1 | 1.24 |
| Periods (C) | .0400 | .0067 | 6 | <1.0 |
| A × B | .0396 | .0396 | 1 | <1.0 |
| A × C | 1.0611 | .1768 | 6 | 1.58 |
| B × C | .6422 | .1070 | 6 | <1.0 |
| A × B × C | .2552 | .0425 | 6 | <1.0 |
| *S*s (A × B) | 29.5627 | .4927 | 60 | |
| C × *S*s (A × B) | 40.3165 | .1120 | 360 | |

* $p < .05$.
** $p < .01$.

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[8] The authors would like to express their appreciation to Arthur Dudycha and Benjamin Winer for their suggestions about analysis and integration of the data.

[9] Some methodological comments about obtaining data as percentages appear in order. Percentage—and other forms of proportion—data may have considerable advantages over data obtained on rating scales. For instance, interval scale assumptions may be more defensible, and forced-choice quantities among various categories may produce more realistic estimates. However, there are two problems with such kind of data. First, percentages adding to unity obtained as a within variable for an analysis of variance design produce identical means for the various levels of other between and within factors, so that other main effects and their interactions with each other should theoretically be associated with an *F* ratio of 1.0. A number of methods can be used to avoid the problem; for instance (*a*) the percentage category of least interest can be dropped from the data; (*b*) a number of percentage categories which are of secondary or joint interest to the researcher may be combined into the mean value of these categories; (*c*) the data from the various percentage categories can be treated separately, and comparisons between main effects and interactions can be made by inspection without testing directly for the percentage-category main effects and its interactions (effects for the percentage measure alone and its interactions may be obtained in a second combined analysis); and (*d*) other (e.g., ordinal) techniques for analysis may be used. Which, or which combination, of the methods is applicable is most profitably determined by the intent of the particular researcher and the requirements of his design. The potential modification of degrees of freedom by some of these methods should, however, be considered.

The second problem with percentage data, which they have in common with all data that are proportions, is their distribution in which means and variances are related. The variances may be stabilized through the transformation $X'_{ijk} = 2\arcsin X_{ijk}$, where $X_{ijk}$ is the original percentage entry in each cell (cf. Winer, 1962).

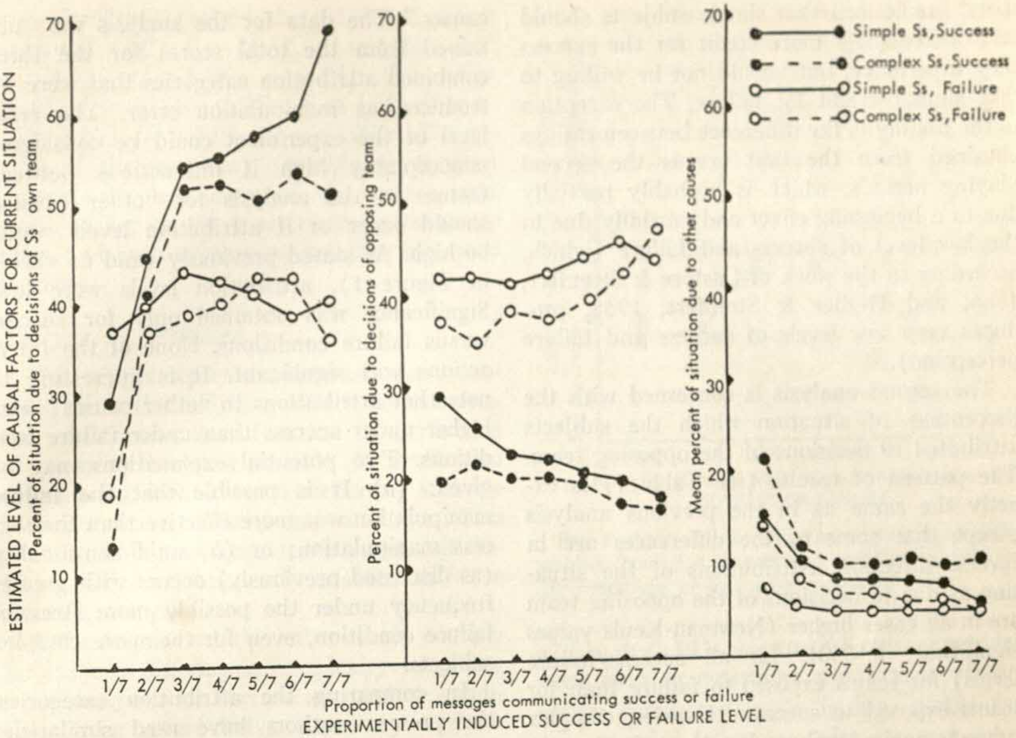

FIG. 1. Attribution of causality for simple and complex subjects under conditions of increasing success or failure.

creasing success or increasing failure listed as proportions of total information) and the percentage estimate of attribution level plotted on the ordinate, is presented in Figure 1.[10]

Before proceeding with a comparison of attribution categories, the authors have interpreted the results obtained for the attribution categories separately. The analysis for the percentage of the situation which subjects attributed to decisions of their own team (Table 1) shows significant $F$ ratios for all three main effects: More causal effect is attributed to decisions of one's own team under success than under failure; simple subjects attribute more than complex subjects; and attributions increase with periods. Two of the interactions also show significance. Attributions to decisions of one's own team increase under success conditions, but not (except from Period 1 to Period 2) under failure conditions, and complex subjects show an initia increase in attribution, but do not maintair the increase as failure or success reache moderate levels. The corresponding post ho analyses based on the analysis of varianc error terms using Newman-Keuls technique bear out the results: attributions under suc cess are higher than those under failure fo Periods 3/7 through 7/7 ($p < .01$). Simpl subjects under success conditions take mor credit for success than complex subject (Newman-Keuls values significant beyond th .05 level for comparisons under 1/7, 5/ 6/7, and 7/7 success levels); however, unde failure conditions simple and complex su jects differ only for the 1/7 failure level. Th results are in agreement with the present a thors' predictions that simple subjects should take increasingly more credit for the success they experience, but should not be willing to take similar credit for failure. The exception to the finding is the difference between ratings obtained from the first versus the second playing periods, which is probably partially due to a beginning effect and partially due to the low level of success and failure (which, according to the work of Castore & Streufert, 1966, and Higbee & Streufert, 1968, produces very low levels of success and failure perception).

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[10] Note that the "percent of situation due to other causes" category is graphically presented as mean percentage of the last three categories combined, so that a representative picture of the size of the specific effects can be gained. For the graphic representation, the mean responses across subjects are maintained as percentage (rather than arcsin values) for the purpose of better communication. It appears unnecessary to present arcsin data since the transformation was made primarily to adjust variance rather than means.

The second analysis is concerned with the percentage of situation which the subjects attributed to decisions of the opposing team. The pattern of results (see Table 1) is exactly the same as in the previous analysis except that some of the differences are in reverse direction. Attributions of the situation as due to decisions of the opposing team are in all cases higher (Newman-Keuls values significant, $p < .01$, for all success/failure levels) for teams exposed to failure than for teams exposed to success. However, complex subjects again attribute less than simple subjects (Newman-Keuls values significant, $p < .05$ for comparisons at 2/7, 4/7, and 5/7 failure levels and at the 1/7 success level). Generally, attributions to decisions of the opposing team decline across playing periods. The decline is produced by teams exposed to success (Newman-Keuls values significant, $p < .05$, for comparisons of 1/7 and 2/7 failure levels with 7/7 and 2/7 with 6/7 level). No differences were found for teams under failure conditions.

Comparing the results for the two analyses, it appears that attributions to either team remain generally constant under failure conditions. However, under success conditions more causality is attributed to decisions of one's own team and less to decisions of the opposing team. Further, it appears that complex subjects generally attribute less causality to either team than do simple subjects. The finding is probably due to the unidimensional "either-or" approach of simple subjects, which should not be shared by their more complex counterparts.

Finally, the authors have interpreted the results obtained for the attributions to "other causes." The data for the analysis were obtained from the total scores for the three combined attribution categories that were introduced as manipulation error. The error level of the experiment could be considered unacceptably high if interactions between factors in the analysis for "other causes" should occur or if attribution levels would be high. As stated previously (and as visible in Figure 1), attribution levels were low. Significance was obtained only for success versus failure conditions. None of the interactions was significant. It is interesting to note that attributions to "other causes" were higher under success than under failure conditions. Two potential explanations may be given: (*a*) It is possible that the failure manipulation was more effective than the success manipulation; or (*b*) unidimensionality (as discussed previously) occurs with greater frequency under the possibly more stressful failure condition, even for the more complex subjects.

In comparing the attribution categories, the present authors have used similarities and differences in the patterns of *F* ratios obtained in the separate analyses. As suggested previously, a combined analysis of all data would not have permitted such comparisons, since the percentage estimates (and, of course, with them the arcsin transformations) added to unity. However, a total analysis, although producing theoretical *F* ratios of 1.0, does provide the opportunity for testing differences among the attribution categories per se.[11] A four-way mixed-design analysis of variance for A (success–failure), B (simple–complex), C (attribution categories), and D (periods) did yield the expected *F* values of near 1.0 for Factors A, B, and D. Factor C, however, yielded an *F* ratio of 34.34 ($df = 1/60$, $p < .01$). The two- and three-way interactions of attribution categories with the other factors were also significant beyond the .01 level, except as noted (AC: $F = 25.47$, $df = 1/60$; BC: $F = 4.94$, $df = 1/60$, $p < .05$; CD: $F = 18.68$, $df = 6/360$; ACD: $F = 4.37$, $df = 6/360$; BCD: $F = 2.97$, $df = 6/360$). The results of the secondary (combined) analysis support the legitimacy of the comparisons made earlier on the basis of the separate analysis of each attribution category by result patterns.

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[11] Degrees of freedom for main effects, interactions, and error terms based on data collected under Factor C were adjusted by dropping 1 *df* since the third attribution category score was determined by the first two. Although this method did not modify the obtained *F* ratios since the mean squares for the factors and interactions are affected in the same way as the associated error terms, it did operate to produce more conservative estimates of significance.

*Analysis 2: Attitudes*

The results reported previously have provided us with conditions where an examination of attitudes as a function of attribution of causality becomes possible. If attribution of causality does act as a mediating variable between success/failure and attitudes, then one should expect considerable parallels between the attribution of causality and the attitude analysis.

Attitudes toward the other team member for subjects differing in complexity–simplicity were analyzed for success and failure conditions in a mixed-design three-way analysis of variance. The results are presented in Table 2. A graphic representation is given in Figure 2.

The predictions of differential attitudes for complex versus simple subjects under success versus failure conditions as success or failure is increasing should produce a three-way interaction between complexity differences (A), success–failure (B), and periods (C). This A × B × C interaction is significant ($F = 17.71$, $p < .01$). Newman-Keuls procedures were used to determine significance of each within category (simple failure, simple success, complex failure, complex success) across the between conditions (periods of increasing success or failure). No differences were found for simple subjects under failure conditions. As causality was attributed externally, attitudes within the group were not modified. The flat curve for such subjects is reminiscent of the findings of Streufert (1966) for simple subjects when interaction distance is varied. The results are also in agreement with theoretical statements on complexity differences.

TABLE 2

ANALYSIS OF VARIANCE SUMMARY TABLE: ATTITUDE ANALYSIS

| Source | *SS* | *MS* | *df* | *F* |
|---|---|---|---|---|
| *M* | 14073.931 | 14073.931 | 1 | |
| Simple-Complex (A—between) | 131.806 | 131.806 | 1 | 35.62* |
| Success-Failure (B—between) | 14.645 | 14.645 | 1 | 3.96 |
| Periods (C—within) | 24.866 | 4.144 | 6 | 15.58* |
| A × B | 6.270 | 6.270 | 1 | 1.69 |
| A × C | 47.304 | 7.884 | 6 | 29.62* |
| B × C | 28.089 | 4.682 | 6 | 17.60* |
| A × B × C | 28.277 | 4.713 | 6 | 17.71* |
| *S*s (A × B) | 222.062 | 3.701 | 60 | |
| C × *S*s(A × B) | 95.749 | .266 | 360 | |

* $p < .01$.

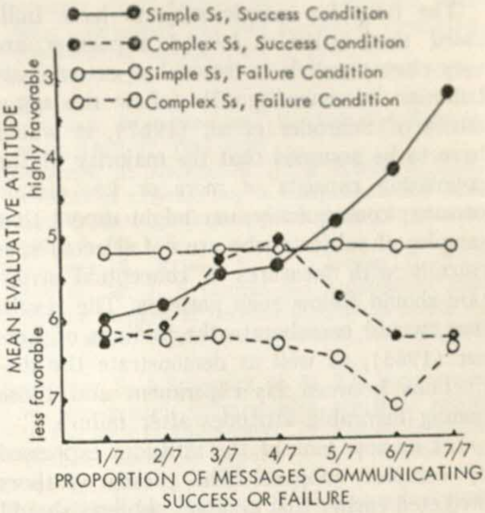

FIG. 2. Attitudes towards the partner in decision-making dyads for simplex and complex subjects under conditions of increasing success or failure.

Quite the opposite should be expected when the credit for success is placed within the group under success conditions. If the partner is credited for his part in group success, his performance should become salient, and attitudes should be modified toward more favorable levels. The graphic representation for simple subjects under success conditions in Figure 2 shows exactly that pattern. Newman-Keuls analysis of the data shows all significant at the $p < .01$ level, except some of the adjacent points on the increasing success dimension: 2/7 versus 3/7, $p < .05$; 1/7 versus 3/7, $p < .05$; 1/7 versus 2/7, *ns;* 2/7 versus 4/7, *ns*; 3/7 versus 4/7; *ns.*

The data for simple subjects have indicated that attitudes toward a partner are very close parallels to internal or external attribution of causality. To follow the arguments of Schroder et al. (1967), it would have to be assumed that the majority of the population consists of more or less simple persons; consequently, one might expect that samples of subjects who are not selected specifically with measures of conceptual structure should follow such patterns. The result, then, would corroborate the findings of Lerner (1965), as well as demonstrate the distinctions between his experiment and those finding favorable attitudes after failure.

Let us now look at the attitudes expressed by complex subjects. The present authors predicted earlier that complex subjects should not place all blame for failure outside the group and should not take all the credit for success. It was found (see Figure 1) that complex subjects (as compared to simple subjects) take less credit for success under extreme success conditions but that there were no differences between simple and complex subjects in taking credit for failure (except for Period 1). What are the effects of such attribution of causality conditions on attitude toward the partner? Under success conditions, we again find a parallel between attribution of causality and attitudes toward the partner. No differences between complex and simple subjects in attitudes toward the partner are evident for 1/7 through 4/7 success levels. Both simple subjects and complex subjects show a significant rise between the two points (Newman-Keuls values significant at the $p < .01$ level except 1/7 versus 2/7 and 3/7 versus 4/7, *ns*). However, beyond that point the direction is reversed. While the attitudes of simple subjects continue to become more favorable, the attitudes of complex subjects begin to drop back until they reach the initial level at 7/7 (Newman-Keuls values significant at the $p < .01$ level except 5/7 versus 6/7, $p < .05$, and 6/7 and 7/7, *ns*). This is exactly the area where significant differences between simple and complex subjects were found in the attribution of causality analysis.

Finally, the authors have considered attitudes of complex subjects under failure conditions. An attribution of causality effect was not found (except for the Period 1–Period 2 differential). In addition, there appears to be little relationship between attribution of causality and attitudes toward the partner. Attitudes remain fairly constant; however, they do show a dip in the unfavorable direction for the 6/7 failure condition. Newman-Keuls analysis shows no significance except between all other conditions with the 6/7 condition ($p < .01$). The present authors offer no explanation of the finding here. Assuming that the result can be replicated, further research is needed to account for this temporary, less favorable attitude.

Generally, it might be concluded that (*a*) as a rule, attitudes are closely related to attribution of causality where present, whether under failure or success conditions; (*b*) blame for failure is placed externally, while credit for success is ascribed to one's own group if conditions permit; and (*c*) simple subjects follow the aforementioned findings more clearly than complex subjects.

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# EXPECTANCY AND FEEDBACK AS INDEPENDENT FACTORS IN TASK PERFORMANCE

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The analysis of the reciprocal effects of expectancy and performance requires that task feedback be made independent of both initial expectancies and actual performance, a condition not met in level-of-aspiration studies. The analysis also requires that some meaningful measure of actual performance be retained, a condition not met in the arbitrary tasks employed in dissonance studies. The present study realized such requirements by using a reaction-time task to explore the independent effects of performance expectancy, feedback, and expectancy changes on performance. After 30 trials of base-line performance, subjects (120 males) either stated performance expectancies (being classified into high-expectancy and low-expectancy groups) or did not give expectancies. Following additional trials without feedback, subjects in each expectancy condition either continued without feedback or received success or failure feedback. Major conclusions were that stating expectancies is a sufficient condition for performance enhancement in the absence of feedback; higher expectancies resulted in greater improvement than lower expectancies. Subjects who resisted lowering their expectancies after failure showed more subsequent improvement than those who lowered their expectancies sharply. Stating expectancies moderated differential reactions to success versus failure, but the crucial dissonance theory prediction, that low expectancy subjects would show a performance decrement under success, was not supported for the reaction-time task.

Cognitive control over behavior leading to success or failure has become an important area of research for dissonance theory since Aronson and Carlsmith (1962) found that people who expected to fail would reject evidence of success. The finding was predicted from the assumption that the disconfirmation of an expectation is itself a source of psychological tension that subjects will strive to avoid or reduce. Evidence in support of the prediction has also been reported by Brock, Edelman, Edwards, and Schuck (1965), Ellsworth (1966), and Winer (1965), while equivocal or negative evidence has been reported by Cottrell (1965), Frentzel (1965), Lowin and Epstein (1965), and Silverman and Marcantonio (1965).

While dissonance theory has stimulated recent interest in the cognitive control of performance, the problem is also of central importance to research on level of aspiration. The analysis of aspirations led the early workers to assign an especially important role to expectations about performance and to the discrepancy between the expectations and actual performance (Festinger, 1942; Lewin, Dembo, Festinger, & Sears, 1944). In fact, aspirations and expectations are not always treated as distinct concepts, and in some cases levels of aspiration have been obtained not by asking subjects to state their goals, but by indicating what level of performance they expected to attain on the next trial. In one of the early studies, Festinger (1942) collected aspiration levels by asking some subjects: "What score do you expect to get next time?" and others: "What score would you like to get next time?" The difference between the questions, considered critical by Festinger in his analysis of aspirations, was one along the reality–irreality dimension. That is, expectations are guided by realistic influences and depend, therefore, to a large extent on information about past performance. But in saying what he would like to score next time, the subject "becomes wishful and unrealistic and raises his goal far above his performance level [Lewin et al., 1944, p. 345]."

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[2] The authors are grateful to James J. Taylor for his help in collecting data.

The basic conceptual differences between performance expectancies and aspirations are not entirely clear. The main difference seems to be one of emphasis. Expectancy places an emphasis on cognitive process while aspiration implies motivational forces at work. It is assumed that setting a more ambitious goal is a sign of a greater motivation to succeed. Subjects who set higher goals for themselves should perform better. Yet, although level of aspiration has traditionally been considered a motivational construct, studies that examine its consequences for performance have been surprisingly rare. As Locke (1966) has pointed out, more is known about the impact of past experience and individual differences on goal setting than about the impact of goal setting on performance. Studies on the latter problem are quite rare (Atkinson, 1964; Rotter, 1954).

Furthermore, it is an oversimplification, and perhaps false as well, to suggest that the concept of aspiration deals primarily with motivational processes while the concept of expectancy deals primarily with cognitive processes. Neither are aspirations purely motivational nor expectancies purely cognitive. First, it has not been demonstrated up to now that setting a goal has *in and of itself* motivational consequences. What studies on level of aspiration have shown (if they have shown performance effects at all) is that *in interaction with past performance*, setting of goals may have influence on future performance. It is entirely possible, therefore, that having or stating aspirations is not a *sufficient* condition for a motivational arousal. Secondly, dissonance theory has been quite explicit in asserting that in general an interaction between cognitions, and in particular a discrepancy between expectancy and cognition about outcome, can be a source of powerful motivational consequences. One cannot exclude the eventuality, therefore, that it is precisely such discrepancies which are the main sources of effects in empirical research on level of aspiration.

Conceptual unclarity about goals and expectancies has been accompanied by (and perhaps has produced) empirical difficulties. Locke has attempted to show that subjects who are led to set specific goals perform better than subjects who are merely told to do their best (Bryan & Locke, 1967; Locke, 1967a; Locke & Bryan, 1966), and that subjects who set hard goals (or are assigned hard goals by the experimenter) perform better than subjects who set easy goals (Locke, 1966, 1967b). Such studies suffer from a drawback that is typical of level-of-aspiration research and a function of the kind of tasks employed: the effects of expectancy or goal-setting on performance are not separated from the effects that derive from feedback and reinforcement. By setting an aspired performance level the person classifies those performances that will be positively reinforced ("successes") and those that will not be reinforced or that will be negatively reinforced ("failures"). For subjects setting high goals (or for those assigned high goals by the experimenter) there exists a narrower range of outcomes that will receive such self-defined or external reinforcement. Hence, we do not know whether in such studies performance effects are due to differential aspirations and expectations, or to differential reinforcement and feedback.

In studies in which the subject is allowed to set his own goals the confounding is further complicated by the fact that goals are influenced by past performance (perhaps the most reliable finding of the level-of-aspiration literature), which in turn is influenced by prior goals and expectancies, which in turn are influenced by self-defined successes and failures. Furthermore, successes are perhaps little more than artifacts of a correspondence (or its absence) between a goal set by the person himself and his level of performance subsequently attained. A failure can be turned into a success and vice versa by a mere change in stated aspirations.

If there is a lesson in the work on dissonance for level of aspiration—namely, the importance of separating feedback from actual performance—there is also a concern of interest to dissonance theory in the work on level of aspiration. Logically, the question of what effects expectancies have on performance under different kinds of feedback requires exploration of the effects of expectancy on performance in the absence of feedback—a necessary control condition to be sure that some performance-expectancy effects would not also take place in the absence of feedback.

The typical dissonance experiment purchases the independence of actual task performance and feedback at a rather high price—using a task on which no objective measures of performance are possible. Thus, in the experiment by Aronson and Carlsmith (1962) subjects were given a test of "social judgment" in which they were asked to pick out the schizophrenics from sets of three photographs—photographs actually taken from the Harvard yearbook and consisting entirely (to the experimenters' knowledge) of normal persons. While the subject changes his responses as a function of feedback, the completely arbitrary criteria of correctness makes it impossible to determine whether the subject's performance *actually* improves or deteriorates in a given condition.

The present study explores the effects of expectancies on performance using a reaction-time task. Although the authors acknowledge that there exists considerable conceptual ambiguity in distinguishing between expectations and aspirations, the present study is limited to the analysis of performance expectancies, leaving for the present time unanswered the important, but empirically more complex, question about aspirations. Nevertheless, many of the conclusions reached apply equally to the concept of aspirations. First, the effects of expectancies in the absence of feedback are explored. Later, when feedback is introduced, the feedback is maintained strictly independent of either subjects' initial expectancies or their actual performances. The nature of the task allows such to be done with a reasonable degree of credibility (as described in the following Method section), while at the same time providing a meaningful record of actual performance changes by subjects under various conditions. Finally, in addition to examining the effects of initial statement of expectancies on performance, the present study also explores how changes in expectancies following feedback predict individuals' future performances.

## METHOD

### *Subjects*

The subjects were 120 males from the University of Michigan subject pool, who were paid $1.25 each for their participation. For the no-expectancy conditions there were 20 subjects in each cell; for the high- and low-expectancy conditions there were 10 subjects in each cell.

### *Apparatus*

The apparatus used in the present experiment was the Group Reaction Time Apparatus (GRTA) described in detail by Zajonc (1965). The apparatus permits several subjects to be run simultaneously, but independently, as in the present experiment. Each subject faces a panel containing a stimulus light, a switch, and a feedback light. Following a warning, the stimulus light flashes, and the subject is to throw his switch as fast as possible while his reaction time (RT) is being automatically recorded. On trials where subjects are to be given feedback, they are told that the feedback light will come on if they fail to react in time—that is, a successful reaction is one fast enough to prevent the red light (a failure signal) from coming on. The failure signal can be programmed by the experimenter.

In the present experiment the warning was given by the experimenter's saying "ready." The interval between the "ready" signal and stimulus onset was varied randomly among 3, 4, 5 and 6 seconds. In all cases the subject's reaction turned off his stimulus light, and his RT was recorded. Trials within a trial block of 10 trials were separated by 20-second intervals, and the trial blocks themselves were separated by 2-minute intervals. Following stimulus onset, the "failure" time was set to 200 milliseconds which is the approximate median reaction time under the previous condition.

### *Procedure*

The details of the design are presented, for reference, in Table 1. In the experiment, all subjects began with three practice blocks, each block consisting of 10 separate reaction-time trials. The subjects undertook the trials prior either to stating expectations for the task or receiving feedback on it. Thus they were able to familiarize themselves with the situation while the experimenter obtained a base-line measure of performance.

After the third trial block half of the subjects were asked to state how many times they expected to succeed in reacting within the permissible interval on the next block of trials (Block 4). All subjects then went through Block 4, still without feedback. Subjects in the high- and low-expectancy conditions again stated their expectations after Block 4.

On Block 5, one-third of the subjects in each group continued as before without feedback. The others were given feedback on each trial via the failure light, one-third receiving low feedback (20% success) and the other third receiving high feed-

TABLE 1

DESIGN OF THE EXPERIMENT

| Feedback for expectancy groups | Trial block | | | | | |
|---|---|---|---|---|---|---|
| | 1–3 | 4 | | 5 | | 6 |
| | Baseline | PE1 | PE2 | F1 | PE3 | F2 |
| High expectancy | | | | | | |
| Success | √ | X | X | + | X | + |
| Failure | √ | X | X | − | X | − |
| None | √ | X | X | 0′ | X | 0′ |
| Low expectancy | | | | | | |
| Success | √ | X | X | + | X | + |
| Failure | √ | X | X | − | X | − |
| None | √ | X | X | 0′ | X | 0′ |
| No expectancy | | | | | | |
| Success | √ | 0 | 0 | + | 0 | + |
| Failure | √ | 0 | 0 | − | 0 | − |
| None | √ | 0 | 0 | 0′ | 0 | 0′ |

Note.—Each treatment is placed at the point in the series of trial blocks when it occurred. PE, performance expectancy; X, stated; 0, not stated; F, feedback; +, success; −, Failure; 0′, no feedback.

back (80% success). The conditions were continued for each group during the last block, Block 6. Failures were distributed randomly within the 10 trials.

Within each feedback condition, subjects who had stated expectancies were divided into high- and low-expectancy groups by splitting them at the median of their prefeedback expectations.

Thus, beginning with Block 5, three expectancy conditions (no expectancy, high expectancy, and low expectancy) are combined with each of three feedback conditions (no feedback, high feedback, and low feedback) in an orthogonal design, yielding nine cells.

## RESULTS

### *Effects of Expectancy on Performance*

The effects of stating performance expectations on subsequent performance, still without feedback, are summarized in Figure 1. The no-expectancy subjects show a small, nonsignificant improvement, while both the high- and low-expectancy groups show substantial improvement. An analysis of variance on the change scores yields an *F* of 7.57, ($df = 2/117$) significant at the .001 level. Individual comparisons on the change scores show that both the high- and low-expectancy groups differed from the no-expectancy group, but not from each other. However, it can be seen from Figure 1 that the subjects who state high-performance expectations for Block 4 (Trials 41–50) are already, during Block 3 (Trials 31–40), performing at a somewhat better level than either of the other two groups, suggesting a possible effect of actual performance, or ability, on expectancy. The difference between the high-expectancy subjects and the others prior to stating expectations was not significant, but since the highs also benefit most from stating performance expectations, the difference between the highs and the lows increases during Block 4. By a Newman-Keuls test, the difference between the high- and low-expectancy subjects on Block 4 is significant between the .10 and .05 levels.

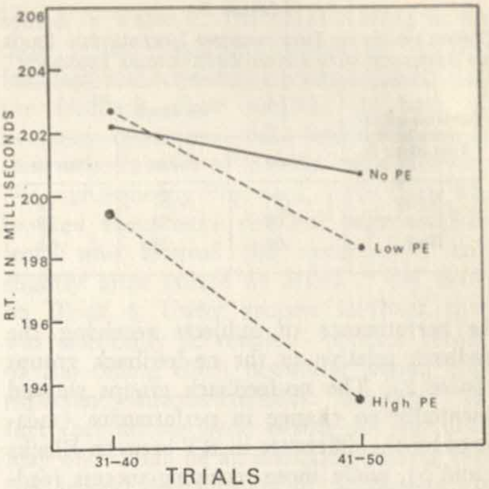

FIG. 1. The effect of stating performance expectancy on reaction time (RT).

### *Effects of Expectancy and Feedback on Performance*

The introduction of feedback on the fifth trial block results in a large improvement in

TABLE 2

MEAN CHANGE IN RT IN MILLISECONDS AS A FUNCTION OF PERFORMANCE EXPECTANCY AND FEEDBACK

| Performance expectancy prior to Trial Block 5 | Feedback | | | All conditions |
|---|---|---|---|---|
| | None | Failure | Success | |
| None | −0.70 | −11.75 | −5.81 | −6.09 |
| Low | −0.50 | −5.97 | −3.72 | −3.40 |
| High | 0.57 | −6.54 | −8.28 | −4.75 |
| All conditions | −0.33 | −9.00 | −5.91 | −5.08 |

TABLE 3

CORRELATIONS OF IMPROVEMENT IMMEDIATELY PRIOR TO FEEDBACK WITH IMPROVEMENT UNDER FEEDBACK

| Performance expectancy prior to Trial Block 5 | Feedback | | |
|---|---|---|---|
| | None | Failure | Success |
| None | −.31 | −.37 | −.19 |
| Low | −.04 | −.58 | −.28 |
| High | .08 | .28 | .61 |

the performance of subjects receiving the feedback relative to the no-feedback groups (Table 2). The no-feedback groups showed essentially no change in performance (measured by the difference in RT between Blocks 4 and 5), while those receiving success feedback showed a mean improvement of 9 milliseconds, and those receiving failure feedback showed a mean improvement of 5.9. Only the main effect of feedback was significant ($F = 10.37$, $df = 2/111$, $p < .001$). The difference between failure feedback and success feedback approaches significance ($F = 3.33$, $df = 1/115$, $p < .10$). Most of the latter effect is contributed by the no-expectancy group, which benefits substantially more from failure feedback than from success feedback. High-expectancy subjects are the only ones showing a tendency to improve more under success feedback than under failure feedback. Subjects who give no performance expectancies or who give low expectancies improve more under failure feedback than under success feedback.

It seemed possible that subjects who had improved more prior to Block 5 and were perhaps closer to their asymptotic performance levels would have a ceiling on their potential improvement during Block 5. An analysis of the Block 5 data covarying improvement during Block 4 was performed. This analysis of covariance did not substantially change the results presented in Table 2. In the course of conducting the analysis, however, it was found that improvement under feedback was related to prior improvement differently in the different conditions. Subjects in the no-expectancy and the low-expectancy groups showed what might be considered a ceiling effect: those who had improved more prior to feedback changed less under feedback. The correlations (over all feedback conditions) for the no-expectancy and low-expectancy groups of prefeedback and postfeedback change were −.30 and −.51, respectively, both significantly different from zero. Subjects in the high-expectancy condition differed from the other two groups, showing a correlation of .32 between prefeedback improvement and improvement under feedback, significant at the .10 level. Of further interest is the patterning of the correlations within the nine conditions. The cell-by-cell correlations are presented in Table 3. No strong faith is to be placed in the exact size of the figures, since many of them are based on only 10 cases. Nevertheless, it is suggestive that the strongest relationships seem to occur in the two consonant cells (for high-expectancy subjects receiving success feedback, and low-expectancy subjects receiving failure feedback).

### *Effects of Feedback on Performance Expectancy*

As could be predicted from the traditional literature on changes in performance expectations (Atkinson, 1964), subjects made large shifts in their performance expectancies following feedback on Block 5. The changes are summarized in Table 4. The main effect of feedback is highly significant, as shown in the analysis of variance presented in Table

TABLE 4

PERFORMANCE EXPECTANCIES AFTER FEEDBACK

| Performance expectancy prior to Trial Block 5 | Feedback | | | Both |
|---|---|---|---|---|
| | None | Failure | Success | |
| Low | 5.1 | 2.2 | 8.0 | 5.1 |
| High | 7.8 | 2.7 | 8.6 | 6.4 |
| Both | 6.5 | 2.5 | 8.4 | |

TABLE 5

ANALYSIS OF VARIANCE FOR TABLE 4

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Expectancy (A) | 1 | 24.07 | 17.91** |
| Feedback (B) | 2 | 178.82 | 133.47** |
| A × B | 2 | 7.72 | 5.74* |
| Error | 54 | 1.34 | |

* $p < .01$.
** $p < .001$.

TABLE 6

MEAN CHANGE IN RT IN MILLISECONDS AS A FUNCTION OF CHANGE IN EXPECTANCY UNDER FEEDBACK

| Performance expectancy | Change in expectancy | Feedback | |
|---|---|---|---|
| | | Success | Failure |
| High | High | 1.10 | 0.96 |
| | Low | 4.10 | −3.88 |
| Low | High | −1.44 | 1.32 |
| | Low | −2.04 | −7.22 |

5. Basically, subjects raised their expectations after success, lowered them after failure, and did not change in the absence of feedback. The fact that the other two effects (the main effect of expectancy and the interaction between expectancy and feedback) are also significant is somewhat misleading, the major contribution to both the effects being the single difference between the high- and low-expectancy subjects who do not receive feedback. This difference is, of course, merely a continuation of the difference that was used to separate the two groups in the first place. Nevertheless, while feedback vastly reduces the differences between such expectancy groups, it does not quite erase them, the high-expectancy group stating slightly higher expectations under both success and failure feedback.

### *Effects of Changes in Expectancies on Performance*

The small adjustments made by subjects in their expectations prior to receiving feedback do not relate to future performance. The changes in expectancy under feedback (following Block 5), however, do relate to performance on Block 6 (measured by difference in RT between Trial Blocks 5 and 6). The results are summarized in Table 6. As was shown in Table 4, subjects receiving feedback tended to shift their expectations such as to bring them closer to the feedback —upward following high feedback, downward following low feedback. The subjects in each condition were split at the median according to the degree of their expectancy shift. The mean performance shift on Trial Block 6 for each group is shown in Table 6, and the analysis of variance for the results is presented in Table 7. The main finding in the table is the significant interaction between feedback and expectancy change. Under failure feedback, those subjects (in both expectancy conditions) who resisted lowering their expectancies, or lowered them relatively less, subsequently improved, while those who lowered them more did not improve. Subjects who lowered the expectancies only slightly after failure on Block 5 did better on Block 6. Under success feedback there was essentially no relation—perhaps because of the ceiling effect obtaining among high-expectancy–success-feedback subjects. The fact that all the main effects are significant may once again be an artifact, resulting from the general deterioration of performance by all subjects in the high-expectancy–success-feedback condition. By Trial Block 6, such subjects are performing faster than subjects in other conditions, and are very likely reaching an asymptotic level.

## DISCUSSION

The present experiment has demonstrated several effects of performance expectancies on performance. First of all, we saw that in the strict absence of feedback, simply stating a performance expectancy is a sufficient condition for performance improvement on a reaction-time task. There are several possible reasons for such an effect. One effect of stating an expectation is to commit a person to the action implied by that expectation. First, it could be that making a commitment, any commitment, is itself a process that enhances motivation and hence improves performance. Secondly, apart from the commitment itself, stating a performance expectancy

TABLE 7

ANALYSIS OF VARIANCE FOR TABLE 6

| Source | *MS* | *F* |
|---|---|---|
| Expectancy (A) | 8,497.2 | 5.45* |
| Feedback (B) | 6,943.2 | 4.45* |
| Expectancy change (C) | 7,535.0 | 4.83* |
| A × B | 2,030.6 | 1.30 |
| A × C | 3,330.6 | 2.14 |
| B × C | 15,563.0 | 9.98** |
| A × B × C | 0.0 | 0.0 |
| Error | 1,560.1 | |

* $p < .05, df = 1/32$.
** $p < .01, df = 1/32$.

can make salient standards of excellence. When performing the task the person may observe and react to various dimensions of the task: the sequence of stimuli, their timing, the nature of the manipulanda, the immediate environment, the topography of his responses, as well as to his successes and failures. Making the latter salient may lead to an increase in achievement motivation; such procedures are in fact used to arouse achievement motivation (see, e.g., Atkinson, 1964).

It is perhaps worth commenting on the fact that people who said they expected to do poorly still improved, relative to controls, almost as much as those who said they expected to do well. It might seem that people committed to low expectations should for the sake of consistency find their improvement inhibited. Either of the aforementioned processes could cancel out such an effect. However, the test of this proposition rests upon the adequacy of the classification of the low-expectancy group. Many of the low-expectancy subjects expected to win 4 times out of 10. It is possible that 4 successes out of 10 was perceived by such subjects as equally challenging and equally rewarding to achieve as the 8 wins out of 10 expected by other subjects. Similarly, later in the experiment, it is possible that winning 4 times out of 10 is more "consonant" with winning 8 times than winning 2 times. No study has yet employed sufficiently sophisticated measurement techniques to know precisely how psychologically discrepant are the various alternatives being tested.

In addition to the effects of stating expectancies, it was also found that changing expectancies related to future performance. Under failure feedback, those subjects (in both expectancy conditions) who did not lower their expectancies showed more improvement on subsequent trials. Thus, while a paradoxical or "atypical" shift after failure—raising one's expectations—may not be a good sign for future improvement, a large downward shift may be equally undesirable. Sharply lowering one's expectancies and improving one's performance may thus be alternative modes of reacting to failure—not parallel ones, as Feather's (1968) data suggested. Feather found that among subjects receiving discrepant feedback, those who changed their expectancies sharply did better than those who did not revise their expectancies so much. However, the present study suggests that lowering one's expectations in the face of failure may hurt, rather than help, perhaps by making failure less incongruent. Of course, the present study and Feather's differed in many respects—including the task (anagram solving vs. reaction time) and the manipulations of success and failure.

Consistency between expectancy level and performance feedback did not produce significant benefits for performance, and inconsistency did not affect performance adversely, though there was a slight trend in that direction. The results do not support the dissonance-theory prediction that low-expectancy subjects would be disrupted by success. For both expectancy groups, the differences between success and failure feedback is slight—in marked contrast to the no-expectancy subjects, who improve much more under failure than under success. It might be noted that previous studies reporting the greater effectiveness of failure feedback employed only no-expectancy subjects (e.g., Johanson, 1922).

If it is assumed that subjects who state low expectancies in an ambiguous situation are lower in achievement motivation and higher in anxiety (see, e.g., Feather, 1965, 1968; McClelland, Atkinson, Clark, & Lowell, 1953; Pottharst, 1955), one would expect such subjects to be more likely to be disrupted by evaluative feedback and, in particular, by feedback indicating that they are failing (Nicholson, 1958; Sarason, Mandler & Craighill, 1952; Weiner, 1966). For such subjects, evaluative feedback would arouse avoidance motivation (fear of failure) that would interfere with performance, while subjects with higher aspirations should be facilitated by such conditions. The data provide even less support for this formulation than for the dissonance predictions.

Two other studies have employed the same reaction-time equipment as the present experiment in somewhat different studies of performance expectancy effects. Burnstein and Zajonc (1965) obtained a dissonance effect, in which subjects who expected to fail attempted to reject discrepant success by slowing down their reaction times. Cottrell (1967), as in the present study, did not obtain such an effect. One difference among the three studies lies in the means employed to induce or infer expectations. In the present study expectations were, of course, elicited from subjects. In the Cottrell study expectations were induced by administering preliminary success and failure treatments to subjects. In the Burnstein and Zajonc study expectations were induced not only by administering appropriate reinforcements but also by assigning subjects either high or low social status in their experimental groups. In the latter study, subjects were thus aware not only of their expectations and perhaps of experimenter's "demands," but also of the expectations of fellow group members. It seems plausible that the strength of the expectations in their study was greater than in either the Cottrell experiment or the present experiment.

There is a suggestion of consistency effects in the present study, however, in the kind of effects suggested by Table 3. The meaning of Table 3 can perhaps be approached from the assumption that success rewards whatever behavior is being emitted, while failure punishes it. Thus, such behavior ought to be continued under success and extinguished under failure. Under success, those who have been concentrating and improving should continue to work and improve; while those who have been relaxing and not improving should continue not to improve—that is, all subjects should continue the behavior that has just been reinforced. This is true for the high-expectancy subjects, but not for the low-expectancy subjects. On the other hand, under failure, those who have been concentrating and working hard may plausibly become discouraged, while those who have been relaxing and not improving may be galvanized into increased effort. This seems true for the low-expectancy subjects, but not for the highs. Thus, each expectancy group seems to respond most "appropriately" to the feedback it expects.

In sum, the present paper has outlined some of the requirements for the study of the cognitive control of performance leading to success and failure and suggested some ways in which such requirements might be met. The authors obtained evidence that performance expectancies enhance performance in the absence of feedback and modify reactions to feedback. The broader question of what kinds of expectancies are optimal for performance in the long run as well as the short run, with differing degrees of risk of failure, remain to be researched.

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# CONTRIBUTION OF GENERALIZED EXPECTANCY TO STATED EXPECTANCY UNDER CONDITIONS OF SUCCESS AND FAILURE[1]

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The present study tests the generality of the proposition from Rotter's (1954) social learning theory that the influence of generalized expectancy (GE) upon expectancy (E) in a noval situation is a rapidly decreasing function of experience in that situation. An analysis was made of the change in correlation between GE (assessed by two methods) and successive E statements elicited after each trial under two sequences of reinforcement on a novel motor-skill task. Under a reinforcement sequence beginning with three failures, GE remained significantly correlated with E over several trials. However, when the sequence of reinforcement began with three successes, the correlation between GE and E fell below statistically significant levels by the second successful trial. When reinforcements are dependent upon the skill of the subject and his skill is sufficient to determine success, a higher order expectancy may be evoked which terminates the influence upon E of expectancies generalized from closely related tasks. Evidence of the construct validity of the measure of E and GE is also presented.

The definition of expectancy used in the present investigation is that of Rotter's (1954) social learning theory. Expectancy (E) is defined as the subjectively held probability that a given reinforcing event will occur in a specific situation contingent upon particular behavior. Two conceptually separate types of experience are considered to determine jointly the value of a given E: the previous experience of reinforcement in the same situation and the history of reinforcement in other situations for functionally related behaviors. The former produces a situationally specific expectancy designated as E′, and the latter produces an expectancy referred to as generalized expectancy (GE).

Of primary concern in the present investigation is Rotter's proposition that the relative contributions to E of E′ and GE change as a function of experience in the situation under consideration. In a novel situation the initial E is determined solely by GE, since no experience has transpired which would provide a basis for determining E′. However, with each successive behavior-reinforcement pairing in the situation, the subsequent values of E are increasingly influenced by the newly emerged E′ and decreasingly influenced by GE. Eventually E′ becomes the major determiner of the current E, while the influence of GE declines to a negligible proportion. As stated by Rotter, ". . . the more the subject tends to differentiate a specific situation from other situations as a result of more experience in that specific situation, the less significant the generalization effect from experience in other situations [1954, p. 166]." The purpose of the present investigation is to test the validity and generality of Rotter's proposition.

There are only a few studies in the literature which bear upon the changing contribution of GE to E. The first, by Dean (1953), used the Rotter Level of Aspiration Board and treated the difference between the total score obtained on the previous block of five trials and the predicted score for the next block (D score) as an index of the influence of GE upon current E. A positive correlation was found between contrived scores delivered earlier on a modified board and D scores at the end of the first block of five trials. Thus, subjects who had received high scores on the prior task tended to state expectancies which were higher than their present level of real

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performance. The relationship never again reached significance for succeeding blocks of trials. Thus, GE had a measurable effect upon current E after five trials, but did not contribute to E on the new task beyond the first block of five trials, which was consistent with Rotter's proposition of a decreasing influence of GE with increased experience. The second study, by Neff (1956), investigated the effects of GE upon persistence in the face of failure on a difficult color-matching task. He found that after all subjects had reached a high level of E, those high in initial expectancy (the index of GE) required more failures to extinguish the E for success. The result suggests that GE was positively correlated with E during the extinction phase of the study. In other words, GE was contributing to the level of E even after a long series of trials on the color-matching task, rather than losing its influence rapidly as proposed by Rotter.

In connection with an earlier experiment (Schwarz, 1966), data were collected which afforded an opportunity to examine the contribution of GE to E on a trial-by-trial basis for two markedly dissimilar patterns of reinforcement on a novel motor-skill task. In addition, two different measures of GE were obtained from each subject, and scores were available on four personality scales which have relevance for the construct validity of the measures of GE and of verbal statements as measures of E. It is the results of the reanalysis of data from the earlier experiment which are presented here.

## METHOD

### *Subjects*

The subjects were 136 male college students enrolled in two lower level psychology courses at Ohio State University.

### *Apparatus*

The novel motor-skill task employed was the Vertical Aspiration Board (VAB—see Rotter, Liverant, & Crowne, 1961, p. 165, for a photograph and description). The apparatus contained a platform upon which a steel ball was placed and raised vertically along a graduated scale by the subject's pulling a cord. Scale graduations were at 10-point intervals from 0 to 100. Unknown to the subject, the ball was held on the platform by an electromagnet; the ball could be released at any point in its ascent by deactivating the magnet. A silent knee switch was situated beneath the table top. The subject was instructed to raise the platform slowly as high as possible before the ball rolled off. Although the experimenter controlled the scores, they were perceived to be the result of the subject's own effort.

### *Measurements*

E was measured immediately before each trial by the subject's choice of a verbal statement of the likelihood of success. Success was defined as reaching or exceeding a score of 80 on the VAB. Subjects were given an 11-point scale, which served as a frame of reference for each E statement. It consisted of a linear scale drawn vertically on a card and graduated at equal intervals with short horizontal lines. Adjacent to the cross marks were statements numbered from 0 through 10 reflecting different levels of E for success. For example, 0 was accompanied by "I am quite sure I shall *not* succeed on this trial." Number 10 was accompanied by "I am very confident that I shall succeed on this trial." The subject simply reported the number associated with the statement which best described his chances of success on the impending trial.

GE for success on the VAB was measured by two methods: One was the subject's first stated expectancy for success preceding the first trial on the novel task ($GE_1$). The only basis for the initial E is what the subject has been told about the task and the expectancies that he generalizes from other situations because of their functional similarity to the experimental task. The other measure of GE, based upon the subject's history of athletic participation, was designed by the investigator in an effort to secure a measure of GE which was relatively free of some of the sources of variation that may influence verbal statements of E, namely, the intersubject variation in caution, social desirability, and conformity. On a three-page questionnaire, subjects listed athletic participation during childhood, high school, and college years; and, for each activity, they rated their degree of proficiency relative to other participants. A scoring manual was constructed which set forth general principles and delineated objective rules for assigning weights to experiences so as to reflect their relevance for the subject's GE for success at athletic skill tasks ($GE_{as}$). Correlating the investigator's scoring of 46 records with that of a graduate student in psychology yielded an interscorer reliability coefficient of .95. (See Schwarz, 1963, Appendix C, for a copy of the history of athletic participation questionnaire and scoring manual.)

Scores were available for most subjects on four other personality measures: the Barron Independence of Judgment scale (BIJ—Barron, 1953), the Marlowe-Crowne Social Desirability scale (M-C SDS—Crowne & Marlowe, 1960), the Internal-External Locus of Control scale (I-E scale—Rotter, Seeman, & Liverant, 1962), and the Rotter Incomplete Sentence Blank (RISB—Rotter & Rafferty, 1950).

TABLE 1

PRODUCT-MOMENT INTERCORRELATION MATRIX OF TWO MEASURES OF GENERALIZED EXPECTANCY (GE) AND FOUR PERSONALITY VARIABLES

| Item | $GE_{as}$ | RISB | BIJ | M-C SDS | I-E scale |
|---|---|---|---|---|---|
| $GE_1$ | .35**<br>(136) | .26*<br>(89) | −.15<br>(134) | .11<br>(122) | −.01<br>(101) |
| $GE_{as}$ | | .08<br>(89) | .11<br>(134) | −.11<br>(122) | −.09<br>(101) |
| Rotter Incomplete Sentence Blank (RISB—adjustment) | | | −.27*<br>(87) | .15<br>(80) | .12<br>(62) |
| Barron Independence of Judgment (BIJ) | | | | −.27**<br>(120) | .02<br>(100) |
| Marlowe-Crowne Social Desirability (M-C SDS) | | | | | −.17<br>(101) |
| Internal-External Locus of Control (I-E scale) | | | | | |

Note.—The *n* for each correlation coefficient is indicated in parentheses. $GE_1$ is the subjects' first stated expectancy for success preceding the first trial on a novel task. $GE_{as}$ is the subjects' generalized expectancy for success at athletic skill tasks.

* $p < .05$, two-tailed test.

** $p < .01$, two-tailed test.

### *Design and Procedure*

The design of the experiment on which the analysis was based (Schwarz, 1963, 1966) consisted of four experimental conditions, the combination of two conditions of spacing of trials and two sequences of reinforcement each 20 trials in length. The data analyzed were the E statements elicited before each of the first eight trials of the reinforcement sequences. The spacing variation was introduced after the eighth E statement; therefore, the spacing variable and the subsequent E statements were disregarded in the analysis.

The history of athletic participation questionnaire was completed by each subject as he arrived individually for the experiment. At this point the subject's only knowledge of the experiment was that it concerned experience and ability. Then the experiment was described as one assessing the level of motor skill of college males, and the VAB was described as the device to be used in the measurements. The task was characterized as "rather difficult," and the total score was said to reflect aptitude for sports and muscular skills. The subject was told that high scores on the motor-skill task were a function of his skill at raising the platform smoothly.

To add to the commonality of perception of the difficulty of the task, subjects were told that the average college student typically achieved scores of 60 or 70. The highest possible score was 100. Reaching or exceeding the 80 mark was designated a "successful" trial. Then the first statement of E, $GE_1$, for success (reaching or exceeding 80) was elicited by means of instructions which described the use of the E scale and cautioned the subject to be realistic in his choice of E statement. After the first trial and each subsequent trial the subject was handed the E scale with the request that he indicate his E for success on the next trial.

### *Reinforcement Conditions*

Subjects were randomly assigned to the two reinforcement conditions. Group A received a sequence of reinforcements that began with three failures (scores of 30 or 40) on the first three trials, followed by one success, another failure, and then two successful trials. Group B received a sequence of reinforcements in which the positions of success and failure were the reverse of that for Sequence A. The order of reinforcement in Sequence B was as follows: three successes, one failure, one success, and two failures.

## RESULTS AND DISCUSSION

Table 1 provides the matrix of intercorrelations among $GE_1$, $GE_{as}$, and the four personality scales. It can be seen that while the correlation between $GE_1$ and $GE_{as}$ was only moderate, its magnitude was within the range of reasonable expectation. The $GE_{as}$ score was a broadly based estimate of GE, derived from a wide range of athletic activities, whereas $GE_1$ reflected only those aspects of each subject's past experience, which, by his idiosyncratic processes of generalization, would be functionally related to the task at hand. None of the personality scales was significantly correlated with $GE_{as}$, thus providing evidence for its discriminant validity. A low, but statistically significant, correlation was found between $GE_1$ and adjustment as measured by the RISB. Since the RISB was not correlated with $GE_{as}$, it appears that the adjustment measure may not reflect GE, but rather a tendency toward optimistic verbalization in unstructured situations. The interpretation was supported by the finding that the relationship between adjustment and E in Condition A (initial failure) dropped from .32 at

TABLE 2

CORRELATIONS BETWEEN ADJACENT EXPECTANCY STATEMENTS IN ORDER OF ELICITATION UNDER TWO CONDITIONS OF REINFORCEMENT

| Condition | $r_{12}$ | $r_{23}$ | $r_{34}$ | $r_{45}$ | $r_{56}$ | $r_{67}$ | $r_{78}$ |
|---|---|---|---|---|---|---|---|
| Condition A (failure–success) | .66 | .84 | .81 | .66 | .73 | .66 | .73 |
| Condition B (success–failure) | .31 | .84 | .85 | .78 | .90 | .85 | .80 |

Note.—$n$ = 68 in Conditions A and B.

$GE_1$ to .00 at $E_2$, the second statement of E, while both $GE_1$ and $GE_{as}$ remained significantly correlated with E after the first trial (see Figures 1 and 2).

Since Dean (1953) and Sears (1940) found curvilinear relationships between measures based on direct experience relating to GE and verbal statements of E, all of the correlations presented in Table 1 were examined for curvilinearity of regression by casting the data in 4 × 4 contingency tables with each variable divided into quartiles. In no case was the relationship observed to be other than linear.

In an effort to obtain evidence bearing upon the validity of the verbal statements of E as measures of the construct E for success, the four personality variables, BIJ, M-C SDS, I-E scale, and RISB, were correlated with E Statements 2 through 8 obtained under Conditions A and B. Out of the resulting total of 56 coefficients only 4 reached or exceeded the .05 level of confidence. The four significant correlations were scattered with no discernible pattern among three of the four personality variables. A systematic check was made for nonlinear relationships for each of the personality variables with three disparate E statements. None was found. It may be concluded that the verbal statements of E elicited under conditions of the present experiment were not influenced by the personality variables measured by the four scales.

Presented in Table 2 are the correlations between adjacent E statements in the order of elicitation in the respective groups. They afford evidence of the reliability of verbal statements of E. The experimental conditions closely approximated the test-retest model for assessing the reliability of measures; however, in this case there was intervening experience, that is, reinforcement, between each measurement of E which would affect the construct being assessed. Therefore, these coefficients may be said to reflect the minimum reliability of verbal statements of E elicited under the conditions of the present study. Since a number of the coefficients were in the 80's, the true reliability of the measures must be quite satisfactory.

Table 3 presents the means and standard deviations of the GE measure and of E statements for Conditions A and B. It may be noted that the standard deviations of E statements remained approximately the same from trial to trial within each condition. This would suggest that neither ceiling nor floor effects restricted the range of E statements on any trial in either condition. The standard deviations of E statements were slightly smaller in Condition A than in Condition B; however,

TABLE 3

MEANS AND STANDARD DEVIATIONS OF GENERALIZED EXPECTANCY (GE) MEASURES AND OF EXPECTANCY (E) STATEMENTS 2 THROUGH 8 FOR SUBJECTS IN CONDITIONS A AND B

| Condition | $GE_{as}$ | $GE_1$ | $E_2$ | $E_3$ | $E_4$ | $E_5$ | $E_6$ | $E_7$ | $E_8$ |
|---|---|---|---|---|---|---|---|---|---|
| Condition A | | | | | | | | | |
| *M* | 8.65 | 3.62 | 4.24 | 3.66 | 3.46 | 5.00 | 4.54 | 5.51 | 6.71 |
| *SD* | 5.39 | 1.69 | 1.39 | 1.43 | 1.49 | 1.51 | 1.52 | 1.38 | 1.58 |
| Condition B | | | | | | | | | |
| *M* | 10.04 | 3.97 | 6.74 | 7.88 | 8.16 | 7.24 | 7.82 | 7.32 | 6.49 |
| *SD* | 5.39 | 1.92 | 1.57 | 1.61 | 1.50 | 1.68 | 1.58 | 1.72 | 1.81 |

Note.—$n$ = 68 in Conditions A and B. $GE_{as}$ is the subjects' generalized expectancy for success at athletic skill tasks. $GE_1$ is the subjects' expectancy for success preceding the first trial on a novel task.

the differences were in a direction which does not alter the interpretation of the subsequent correlational analysis.

In Figure 1, coefficients of correlation between $GE_1$ and subsequent E statements are plotted separately for Conditions A and B and as a function of reinforced trials. It can be seen in Figure 1 that $GE_1$ was significantly correlated with $E_2$ under both reinforcement conditions. For Group A, which received the failure-success sequence, $GE_1$ remained significantly correlated with E through statement $E_7$; its correlation with $E_8$ did not reach the .05 level of confidence. By contrast, in Group B, which received the success-failure sequence, the correlations of $GE_1$ with $E_3$ and $E_4$ were not significant, but the correlations with $E_5$, $E_7$, and $E_8$ which followed negative reinforcements were statistically significant. At every point except $E_8$ the observed correlation of $GE_1$ with subsequent E statements was greater in Group A (failure-success) than in Group B; however, the differences between the two conditions only reached statistically reliable magnitudes at $E_2$ ($z = 2.67$, $p < .01$)[3] and $E_3$ ($z = 2.50$, $p < .02$).

Of major concern in the analysis was the change over trials in the contribution of GE to E. It seems reasonable to assume that the $GE_1$ score was equally valid in Conditions A and B as a measure of the construct GE. Therefore, the difference in the magnitude of the $GE_1$–$E_2$ correlations between Groups A and B (plotted at $E_2$ in Figure 1) indicated different degrees of change in the contribution of GE to E. In the case of the condition beginning with success (Condition B) there appears to have been a substantial drop in the contribution of GE to E from $GE_1$ to $E_2$, whereas in the condition beginning with failure (Group A), it may be inferred that the drop was smaller. However, on subsequent E statements in Condition A there were further reductions in the contribution of GE to E as indicated by the general decline of the curve of correlations. In Condition A the differences between the correlations of $GE_1$ with $E_2$, with $E_3$, and with $E_4$ were all significant at the .05 level or better (test for correlated coefficients of correlation, Guilford, 1956, pp. 193–194). Also, in Condition B the contribution of GE to E continued to drop beyond the second E statement; the difference between the $GE_1$–$E_2$

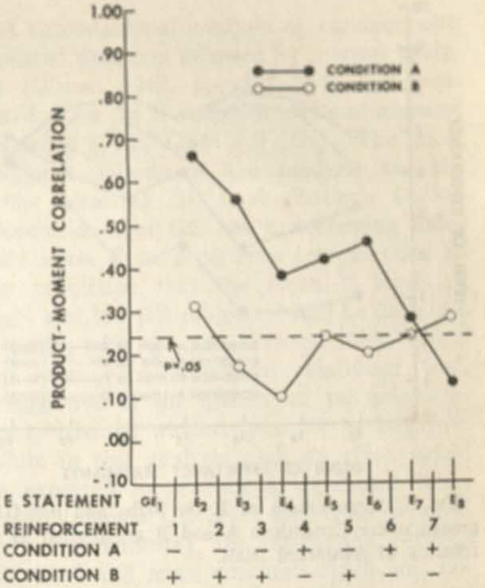

FIG. 1. Product-moment correlations between $GE_1$ and subsequent E statements are plotted separately for Conditions A and B as a function of reinforced trials.

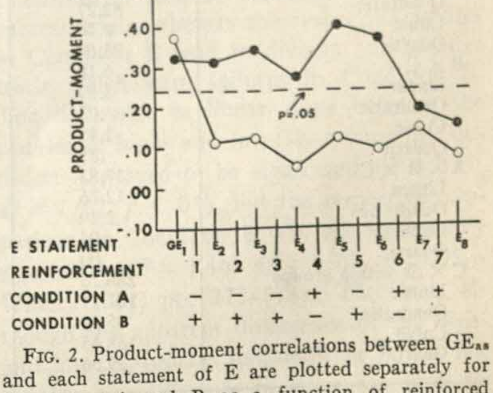

FIG. 2. Product-moment correlations between $GE_{as}$ and each statement of E are plotted separately for Conditions A and B as a function of reinforced trials.

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[3] Two-tailed tests are used throughout.

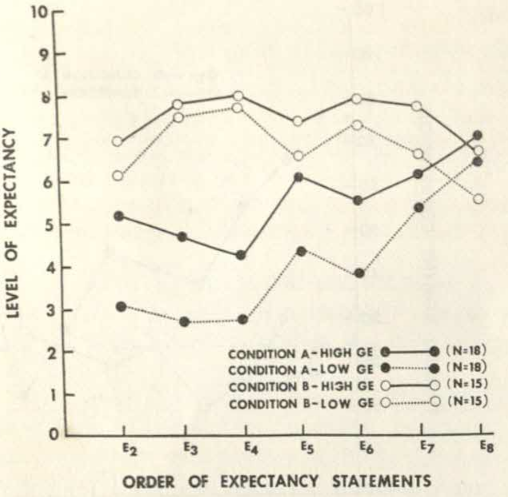

FIG. 3. Mean values of E for high- and low-GE groups under Conditions A and B are plotted as a function of reinforced trials.

correlation and $GE_1$–$E_3$ correlation was also significant ($z = 2.21$, $p < .05$). However, after the first failure in Condition B, there appeared to be an increase in the contribution of GE to E, but the difference between the $GE_1$–$E_4$ and the $GE_1$–$E_5$ correlations did not quite reach significance ($z = 1.77$, $p < .08$). The results with the $GE_1$ measure provided some evidence that successful experience on the skill task reduced the influence of GE upon E, whereas the absence of success tended to continue or renew the influence of GE upon E.

In Figure 2, coefficients of correlation between $GE_{as}$ and each E statement including $GE_1$ are plotted separately for Conditions A and B as a function of reinforced trials. The curves for Conditions A and B stand in roughly the same relation to one another as

TABLE 4

ANALYSIS OF VARIANCE OF EXPECTANCY (E) STATEMENTS AS A FUNCTION OF REINFORCEMENT SEQUENCE AND LEVEL OF GENERALIZED EXPECTANCY (GE)

| Source | *SS* | *df* | *MS* | *F* |
|---|---|---|---|---|
| Between *Ss* | | | | |
| Reinforcement sequence (A) | 611.39 | 1 | 611.39 | 48.65* |
| GE level (B) | 146.32 | 1 | 146.32 | 11.64* |
| A × B | 17.82 | 1 | 17.82 | 1.42 |
| *Ss* within groups | 779.17 | 62 | 12.57 | |
| Within *Ss* | | | | |
| E statements (C) | 87.50 | 6 | 14.58 | 18.92* |
| Linear | 83.15 | 1 | 83.15 | 48.12* |
| Quadratic | .01 | 1 | .01 | <1 |
| Cubic | .25 | 1 | .25 | <1 |
| Quartic | .29 | 1 | .29 | <1 |
| A × C | 291.18 | 6 | 48.53 | 62.96* |
| Linear | 139.90 | 1 | 139.90 | 80.96* |
| Quadratic | 82.27 | 1 | 82.27 | 112.51* |
| Cubic | 2.46 | 1 | 2.46 | 3.97 |
| Quartic | 25.69 | 1 | 25.69 | 53.89* |
| B × C | 6.19 | 6 | 1.03 | 1.34 |
| Linear | 3.29 | 1 | 3.29 | 1.91 |
| Quadratic | .03 | 1 | .03 | <1 |
| Cubic | 1.45 | 1 | 1.45 | 2.35 |
| Quartic | .35 | 1 | .35 | <1 |
| A × B × C | 17.83 | 6 | 2.97 | 3.86* |
| Linear | 12.76 | 1 | 12.76 | 7.38* |
| Quadratic | 2.49 | 1 | 2.49 | 3.40 |
| Cubic | .04 | 1 | .04 | <1 |
| Quartic | .00 | 1 | .00 | <1 |
| C × *Ss* within groups | 286.75 | 372 | .77 | |
| Linear | 107.11 | 62 | 1.73 | |
| Quadratic | 45.33 | 62 | .73 | |
| Cubic | 38.45 | 62 | .62 | |
| Quartic | 29.56 | 62 | .48 | |

* $p < .01$.

was found in Figure 1. Under Condition A, which began with failure, the correlations of $GE_{as}$ with E remained statistically significant up to $E_7$ and $E_8$, the E statements which were preceded by success. On the other hand, under Condition B, which began with success, at no point was the correlation between $GE_{as}$ and a postreinforcement E statement statistically significant. The maximum differences between Conditions A and B in relative contribution of $GE_{as}$ to postexperience E statements occurred at $E_5$ and $E_6$. These differences did not reach acceptable levels of significance ($p < .09$ in each case).

For both samples $GE_{as}$ was significantly correlated with $GE_1$ and to approximately the same magnitude, that is, .32 for Condition A and .37 for Condition B. It may be recalled that the $GE_{as}$ was a broadly based measure of GE and, hence, was not expected to bear a high relationship to the task-specific construct of GE assumed to be operating in subjects in the present study. With this idea in mind, it is interesting to note that in Condition A, $GE_{as}$ did not undergo a reduction in its contribution to E after experience which failed to produce success. On the other hand, when subjects were initially successful, as in Condition B, the difference between the $GE_{as}$–$GE_1$ correlation and the $GE_{as}$–$E_2$ correlation approached significance ($z = 1.89$, $p < .06$) and was in sharp contrast with the respective comparison in Condition A.

High- and low-GE groups were created by placing those subjects who were above the mean of the total sample on both $GE_1$ and $GE_{as}$ in the high group and those below the mean on both measures in the low group. Such a division yielded 18 high- and 20 low-GE subjects in Condition A and 22 high- and 15 low-GE subjects in Condition B. In order to achieve proportional group sizes for analysis of variance, two subjects were randomly deleted from the Condition-A–low-GE group, and seven were deleted from the Condition-B–high-GE group. The mean E statements of the resulting four groups are plotted in Figure 3 as a function of reinforced trials. Thus, Figure 3 shows the gross effect of the two sequences of reinforcement and the influence of high- and low-GE levels upon E within each condition of reinforcement.

A three-factorial analysis of variance with repeated measures followed by a trend analysis (Winer, 1962, pp. 337–369) was computed upon the E statements (Reinforcement Sequence × GE Level × Trials). The most pertinent aspects of the analysis are the terms involving GE level. Rotter's (1954) proposition that GE has a decreasing influence upon E as experience increases led to the prediction that the mean E levels of high- and low-GE subjects would be disparate initially, with the gap between them lessening after each successive reinforced trial, irrespective of the quality of the reinforcement, that is, whether positive or negative. While in this analysis such an effect would be expected to produce a significant interaction between GE level and trials (successive E statements), the interaction between GE level and trials was not significant, nor was the interaction between GE level and the reinforcement condition. (See Table 4 for a summary of the analysis of variance.) However, there was a significant third order interaction ($p < .01$) involving GE level, trials, and reinforcement conditions, which indicated that the effect of the subject's GE level upon subsequent expectancies was a joint function of the number and quality of the reinforced trials that he experienced in the situation. The analysis of trend effects for the third-order-interaction term showed that the linear effect was significant, implying differences in linear slope among the four profiles of E statements. Figure 3 suggests that the profiles of the high- and low-GE groups in Condition A tend to converge on the later success trials, whereas the respective profiles in Condition B tend to diverge on the later trials which were failures in Condition B. The difference in linear slope between the profiles of high- and low-GE groups in Condition A proved to be significant ($F = 8.10$, $df = 1/62$, $p < .01$), but the respective comparison on Condition B was not statistically significant ($F = 1.46$, $df = 1/62$, $p > .05$—Winer, 1962, pp. 273–278). Therefore, although the apparent divergence of the high- and low-GE profiles in Condition B was not reliable, the difference in slope was in a direction opposite to the prediction of convergence which follows from Rotter's proposition.

## CONCLUSIONS

The results of the analysis of variance are in accord with the correlational analysis. Both suggest that the contribution of GE to E is not a simple function of the number of trials experienced in the situation, but rather the contribution is influenced by the quality and the pattern of reinforcements. In the present study, under conditions of initial failure, subjects tended to retain their relative and absolute positions with regard to E level from $GE_1$ until they met with success, which then produced a new ordering. Under the condition of initial success, after the first trial there were dramatic upward shifts in the overall level of E and a reordering of relative position in E level, which tended to be retained over subsequent trials.

Further investigations of this problem with other tasks are required to assess the generality of the differences observed here between initially successful and initially unsuccessful experience. There is good reason for caution. Rotter et al. (1961) found marked differences in rate of change in E between a skill- and a chance-controlled task when the same sequence of reinforcement was employed with each. Expectancies rose more rapidly with success and were much more resistant to extinction after 100% reinforcement on the skill task than on the chance-controlled task. They reasoned that subjects experiencing the skill task had become convinced that they possessed the necessary skill. It may be hypothesized that initial success on a novel skill task evokes a higher order GE of a conditional nature which is applicable to skill tasks in general; namely, an expectancy that when reinforcements are dependent upon the skill of the subject and his skill is sufficient to determine success, prior experience on related tasks no longer influences the E for future success on the task at hand. Just what constitutes a demonstration of sufficient skill to control reinforcements may vary, independently of subject differences, from one task to another. The more clearly success is a function of skill, rather than chance or external forces, the fewer consecutive successes may be required to void the influence of expectancies generalized from closely related tasks. Another factor influencing the perception of sufficient skill to control reinforcement may be the size of the margin by which performance exceeds the criterion of success. If success is not forthcoming on the initial trials of a skill task, expectancies generalized from related situations may continue to influence the E for success.

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# TEST OF THE "RISK IS A VALUE" HYPOTHESIS[1]

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The study aims to provide a new test of Brown's "value theory" for understanding the risky-shift phenomenon. Two hundred fifty subjects in three samples responded to the Kogan-Wallach choice dilemmas in three ways: (*a*) their own choice, (*b*) what they believed their typical fellow student would choose, and (*c*) the choice they themselves would most admire. It was found that on most of the 12 items the average subject (*a*) believed "fellow students" to be more conservative than himself, and (*b*) most admired a choice more risky than his own. These results were *not* obtained on items that in previous research have not consistently shown a shift toward risk. The data support a value interpretation of the risky-shift phenomenon; they are also interpreted to indicate the existence of a more general phenomenon that might be labeled the "choice-shift."

Gordon Allport once wrote "If we want to know how people feel, what they experience and what they remember . . . why not ask them?" For a number of years, there has been debate about Brown's (1965) suggestion that the "risky-shift" phenomenon is in part a function of subjects' perception of a cultural value in risk taking. There has still been no direct test of that suggestion. In this paper findings are reported concerning subjects' reported value for risk taking. The value theory will also be considered as it might pertain to more general group effects on certain kinds of individual judgments.

Various investigators (e.g., Kogan & Wallach, 1967b; Stoner, 1961; Teger & Pruitt, 1967; Wallach, Kogan, & Bem, 1962) have shown that group discussion generally leads to a group decision more risky than the average of the prediscussion individual decisions, but there is as yet no agreement why this should occur. A variety of hypotheses about the occurrence of the risky-shift phenomenon have been advanced, and some have already been disconfirmed (cf. Kogan & Wallach, 1967a; Pruitt & Teger, 1967). Currently the two most prominent explanations, which are not mutually exclusive, are the "diffusion of responsibility" and the "risk is a value" hypotheses.

The first explanation, endorsed principally by Kogan and Wallach (Wallach & Kogan, 1965; Wallach, Kogan, & Bem, 1962), suggests that in relatively large groups the members are more inclined to support risky decisions, since responsibility for failure is spread among the group members. Diffusion of responsibility would weaken members' concern with the negative aspects of the risky decision, and therefore would enhance its attractiveness. And, accordingly, it has been found that the shift is more likely to be found in larger groups, in more cohesive groups, for more anxious people, etc. (Kogan & Wallach, 1967a; Wallach & Kogan, 1965; Wallach, Kogan & Bem, 1964). Why on some items groups move predictably *away* from risk is left unexplained. Furthermore, there is other experimental evidence which the responsibility diffusion theory cannot explain satisfactorily (Pruitt & Teger, 1968).

The second explanation, offered by Brown (1965) and by Teger and Pruitt (1967), is based on two not necessarily related assumptions: (*a*) that people in our society value risk and (*b*) that most people feel they themselves are greater risk takers than the average person. According to this second line of reasoning, group discussions would provide information to the ignorant average group member, allowing him to discover that he is not as risky as he had previously thought. If the first assumption holds (that risk is positively valued), then the average member should become more risky after learning that

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he is relatively unrisky—either to maintain his self-image as a "more than average risky" person or because he has established a new frame of reference of what is a "risky" decision.

Both Teger and Pruitt (1967) and Kogan and Wallach (1967b) have shown that information exchange per se—without group discussion—leads to a significant shift. This result lends some circumstantial support to the value hypothesis, since it can be argued that as soon as the typical person discovers that he is only average in his risk proneness, he moves over to a more risky stance. Nevertheless, since active group discussion had led to a greater risky shift than simple information exchange in these studies, the "risk as value" hypothesis in its present formation is probably not the sole explanation for the phenomenon.

More recently, the value hypothesis has received some support from Wallach himself. Wallach and Wing (1968) found that on a set of 6 items of the 12-item choice-dilemma questionnaire, Duke University male and female undergraduates saw themselves as typically more risky than "other Duke students." Their evidence extended previous pilot results of Hinds (1962) and supported one of the two major assumptions of Brown's (1965) value theory—that others are perceived erroneously to be less risky than the subject himself.

However, Wallach and Wing did not employ two of the remaining six choice dilemmas (Items Number 5 and 12) on which groups usually shift in a cautious direction. If Brown's theory is correct, then on those items the distortion of the position of the "typical other" should be much less. One purpose of this study, therefore, was to extend Wallach and Wing's findings, using *all 12* of the choice dilemmas as well as several new samples of subjects.

A second, more important purpose was to provide direct evidence for the assumption that risk taking is positively valued. It was reasoned that wherever risk is a positive value, then the choice of odds the subjects admire most would be more risky than their own choice.

## METHOD

### Subjects

The subjects were 250 undergraduates enrolled in psychology classes at either Amherst College or the University of Massachusetts. At Amherst, 99 males participated; at Massachusetts there were 83 male subjects and 68 female subjects. Subjects completed the questionnaire during either a regular class or a laboratory period.

### Procedure

Each subject read the 12 situations from Kogan and Wallach's (1964) choice-dilemma instrument. In each of these situations, the central person is faced with a choice between two alternative courses of action, which might be called X and Y. Alternative X is more desirable and attractive than alternative Y, but the probability of attaining or achieving X is less than that of attaining or achieving Y.

Subjects responded to the 12 situations from each of three different standpoints. Instructions for Forms A, B, and C were as follows:

> A. Imagine that you are advising the person making the choice. Try to place yourself in the position of this central person. Indicate the minimum odds of sucess *you* would demand before recommending that the more attractive or desirable alternative be chosen. [These instructions parallel the ordinary ones for the choice dilemmas, given by Kogan & Wallach, 1964.]
>
> B. You have just finished describing the odds for success which you would consider acceptable. Now reread each situation and indicate on this sheet the answers which you believe a *majority of your fellow students here* would mark for each item. For each situation, indicate the lowest odds which the majority of your fellow students would want before advising that the central person choose the more attractive alternative. [This is a paraphrase of Wallach & Wing, 1968, p. 104.]
>
> C. Please reread each situation for a third and last time. This time, indicate the choice you would *admire most,* regardless of what you or most others might prefer. For *each* situation, what choice of odds would you consider most admirable? [New instructions.]

Respondents completed each of three answer sheets in turn, answering each of the 12 items sequentially. After finishing Answer Sheet A, the subject returned it and completed Answer Sheet B without looking back to his previous answers. After Sheet B had been returned, Answer Sheet C was filled out in the same fashion.

## RESULTS

Table 1 presents means for all 12 items for each of the three answer forms.[2] Table 2 is

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[2] Separate data analyses for the three samples of (*a*) 99 Amherst males, (*b*) 83 Massachusetts males,

TABLE 1

MEANS FOR AMOUNT OF RISK (A) CHOSEN BY SELF, (B) EXPECTED TO BE CHOSEN BY FELLOW STUDENTS, AND (C) ADMIRED MOST

| Choice dilemmas | Own choice | Fellow students | Admired most |
|---|---|---|---|
| 1. Engineer's job | 5.29[a] | 5.80 | 4.72 |
| 2. Heart operation | 6.79 | 7.38 | 5.51 |
| 3. $4000 investment | 5.60 | 5.56 | 5.32 |
| 4. Football play | 4.21 | 4.55 | 3.21 |
| 5. Corporation plant location | 7.11 | 7.01 | 6.21 |
| 6. Graduate school choice | 5.30 | 6.52 | 3.58 |
| 7. Chess play in tournament | 4.26 | 4.81 | 3.41 |
| 8. Medical school vs. concert piano | 5.87 | 6.72 | 4.13 |
| 9. POW escape attempt | 5.24 | 5.88 | 3.99 |
| 10. Run for political office | 5.47 | 5.73 | 4.20 |
| 11. Several short vs. one long problem | 4.17 | 5.80 | 3.02 |
| 12. Marriage for Mr. M and Miss T | 7.31 | 6.90 | 7.02 |
| Total *M* | 5.55 | 6.06 | 4.53 |

[a] Odds out of 10 acceptable: the smaller the number, the more risky the odds.

derived from the results shown in Table 1. The left-hand column of Table 2 shows the mean differences between the subjects' own responses (Form A) and their assumed others' responses (Form B). Negative differences would indicate that the average subject saw himself as more risky than the majority of his fellow students. The findings indicate that on most of these choice dilemmas the differences between Forms A and B were significantly negative. The exceptions occurred on three items: 3, 5, and 12; on Item 12, subjects believed that others would be *significantly more* risky than themselves. These findings were generally in accord with the hypothesis, and also with Wallach and Wing's (1968) reported results.

The right-hand column of Table 2 shows the mean differences between responses to Forms A and C. As would be predicted by a value theory, the differences were significantly positive for almost all items. The exceptions were Items 12 and 3, two of the three deviant items in the previous comparison.

## DISCUSSION

The results of this study are consistent with a "value theory" interpretation of the risky-shift phenomenon. On the one hand, these subjects perceive themselves as significantly more risky than their typical peer; on the other hand, they see the most admirable choice as more risky than their own. The first finding supports the assumption that people believe erroneously that they are more risky than others, and the second that they do value the more risky choices.

Therefore, the average subject, who values a decision more risky than his own and who sees himself as more risky than average, might become more risky when he discovers his false assumptions about others. We say "might" because this paper does not present evidence that such a process does actually occur. This study does, however, provide the

TABLE 2

DIFFERENCES IN MEAN AMOUNT OF RISK (A) CHOSEN BY SELF, AND EITHER (B) EXPECTED TO BE CHOSEN BY FELLOW STUDENTS OR (C) ADMIRED MOST BY SELF

| Items[a] | Own choice–Fellow students[b] | Own choice–Admired most |
|---|---|---|
| 1 | −.51*** | .57*** |
| 2 | −.59*** | 1.28*** |
| 3 | +.04 | .28 |
| 4 | −.34* | 1.00*** |
| 5 | +.10 | .90*** |
| 6 | −1.22*** | 1.72*** |
| 7 | −.55*** | .85*** |
| 8 | −.85*** | 1.74*** |
| 9 | −.64*** | 1.25*** |
| 10 | −.26* | 1.27*** |
| 11 | −1.63*** | 1.15*** |
| 12 | +.41** | .29 |
| Total *M* difference | −.50 | 1.02 |

Note.—*p*'s determined by *t* test of difference between correlated means for a combined sample of 250 subjects.

[a] See Table 1.

[b] Differences refer to means in Table 1.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

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and (*c*) 68 Massachusetts females showed very similar (and significant) results for each single group. There were no systematic variations among the three samples.

There were consistent differences in the size of the standard deviations across the three answer forms. Items on Form B (fellow students) showed consistently lower standard deviations than those on Form A (own choice); 33 of the 36 standard deviations for the three samples on the 12 items were smaller on Form B than on Form A. Similarly, 35 of the 36 standard deviations were lower for Form B than for Form C. Comparing the standard deviations between Forms A and C, those for Form A were lower in 26 out of 35 instances. In other words, the subjects showed greatest variability in what they "admire most," medium variability in their own choices, and least heterogeneity in expectations of their fellow students' choices. Perceived consensus about others' choices was greater than similarity in the actual choices themselves.

A table containing these detailed data is obtainable from the authors upon request.

first direct test of both of the assumptions underlying the value hypothesis. The next step would be to determine whether the actual process of group discussion is affected by the degree to which risk is valued and by the extent to which group members are ignorant of the riskiness of others.

Although data from actual group discussions are not reported here, an indirect test of the hypothesis can be made. If the positive-value hypothesis correctly explains the risky-shift phenomenon, then it would follow that on those items where previous studies have *not* observed a shift toward risk one would be less likely to find either admiration for high risk or an attribution of greater conservatism to one's fellow students.

Consider Item 12 as an instance: Should Mr. M and Miss T marry, despite having received advice from a marriage counselor that their marriage would be a chancy undertaking? On this exceptional item, previous studies have tended to find a shift in the cautious direction (cf. Brown, 1965; Teger & Pruitt, 1967; Wallach & Wing, 1968). It may be seen in Table 2 that, indeed, for this item subjects (*a*) attributed to others greater relative riskiness rather than greater caution, and (*b*) did not admire a significantly greater amount of riskiness than they themselves had chosen.

Two additional items—Numbers 3 and 5—did not follow the general pattern of the other items in this study. In previous risky-shift research, Item 5 is the only other item which has consistently generated shifts in the cautious direction (cf. national norms reported by Pruitt & Teger, 1967). According to these same norms, Item 3 has shown the second least shifting toward risk among the remaining 10 items. This indirect analysis, then, indicates that the present findings are consistent with some inferences about the risky-shift phenomenon.[3]

*Is there a general "choice-shift" phenomenon?* The present findings led the authors to reexamine the so-called risky-shift phenomenon in terms of more general principles that may apply to other sorts of group effects. Assume that choices generally are compromises between two conflicting pressures: one, what the individual values or desires; and the second, what he considers realistic or reasonabe. In determining what is realistic, other persons serve as a reference group that has a comparison or information function (Kelley, 1952). A conflict-compromise model, then, predicts that new information, considered in such a group context, systematically affects an individual's expressed choices.

Consider a group's discussion of a heavily shifting item. Having earlier indicated his own choice, an average group member discovers that other members stand closer to his ideal point than he had earlier believed; his fellow members simultaneously make the same discovery. Thus the group's realistic reference point is shifted closer to the mean ideal point. Members are freed to make a new compromise between their ideal and the newly perceived point of reference, and their choices can shift closer to their ideal. This logic would apply to predictions about shifts in any direction, toward either relatively risky or relatively conservative ideal points. The findings of the present study would support such a model, but further experimentation is necessary to test such a post hoc interpretation.

If it is correct, this reasoning would be applicable to group effects on dimensions other than that of riskiness. The risky shift may be only one example of a more general phenomenon. Perhaps this phenomenon should be relabeled the "choice shift."

*"Choice-shift" effect.* It is hypothesized that this effect occurs whenever (*a*) a group member has an ideal preference that is discrepant from what he believes to be the group standard, (*b*) his own choice concerning real behavior lies between his ideal and his assumed group standard, and (*c*) a group discussion (or some other information) reveals that the group standard is substantially

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[3] Unfortunately, however, the results for the remaining nine items were not entirely consistent with previous risky-shift results. Not all items on which risk was relatively admired have, in previous research, shown strong and consistent risky shifts; nor, on those items where previous studies have consistently found such shifts, did the authors necessarily find the greatest admiration for risk. Thus it is apparent that Brown's "risk as value" hypothesis probably does not fully explain the phenomenon. Pruitt and Teger (1967) also have recently arrived at this tentative conclusion on the basis of other data and a different line of reasoning.

closer to (or farther from) his ideal than he had earlier believed.

Any valued choice characteristic potentially would fit this model. For example, altruism, intellectualism, drinking behavior, and so forth could also show shifts toward the individual's private values after group discussion. Predictable choice-shift effects may be found on a variety of choice dimensions not as yet investigated.

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# EFFECTS OF ALCOHOLIC BEVERAGES AND THEIR CONGENER CONTENT ON LEVEL AND STYLE OF RISK TAKING

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The risk-taking behavior of sober subjects on the choice-dilemma questionnaire was compared to that of subjects drunk on bourbon, vodka, and synthetic alcohol. The results indicated that (*a*) contrary to previous findings, drunk subjects will accept greater subjective risk than sober subjects, and (*b*) subjects drunk on bourbon, which is very high in congener content, will take greater risk than those drunk on the other two beverages, which are low in congener content. Alcohol produced greater elevation on some items in the questionnaire than on others. Earlier research has shown that older age produces a decrease in risk on the same items, while group discussion produces an increase in risk on a different subset of items. This may indicate, among other things, that the shift toward greater risk taking that occurs under the influence of alcohol is not produced by the processes that are responsible for the risky shift in group discussion.

The popular assumption that people are more willing to take a risk when drunk than when sober has been tested by Cohen (1960) in a study of the judgment and driving ability of bus drivers. In Cohen's study the drivers sat in their buses while the experimenter moved two large poles to various size openings without announcing the size of the openings. The subjects were first asked to estimate their probability of success in driving the bus through the various openings. They were then asked to choose the smallest opening through which they would attempt to drive. Finally their actual performance was measured at various openings. Cohen found that drivers drunk on Scotch tended to make larger estimates of their likelihood of success than sober drivers. Drunk drivers also were willing to drive through narrower openings than sober drivers, and in so doing, had a lower objective chance of success than sober drivers. Nevertheless, it was found that both kinds of drivers had initially estimated the same average probability of success (56%) for the openings which they later were minimally willing to attempt.

Cohen concluded (*a*) that alcohol distorts the *perception of how much risk is being taken* and (*b*) that alcohol does not affect the willingness to take risk (i.e., does not influence the maximum level of subjective risk people are willing to take). The first conclusion seems securely anchored in Cohen's data, but the second conclusion is less secure, because the methodology underlying it was not very rigorous. The second conclusion, that alcohol does not affect the willingness to take risk, was based on the assumption that drunk and sober subjects had an equal subjective probability of success at the time of their decisions about the minimally acceptable opening. Yet the evidence came from the beginning of the experiment, well before these decisions were made and probably before they were even contemplated; and it is not unreasonable to suppose that the subjective probabilities changed subsequently, under the pressure of the necessity to make a decision. Thus, there are reasons to question Cohen's assumption that the drunk drivers perceived the same probability of success as the sober drivers at the point of decision about the smallest opening through which they would drive; hence his conclusion that alcohol does not affect the willingness to take risk may also be questioned.

One purpose of the present experiment was to employ an alternative technique for studying the willingness to take risk—one in which the actual probability of success was known by the decision maker and hence not subject to distortion; in other words, a task in which objective and subjective risk were equivalent.

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[1] Requests for reprints should be sent to Allan I. Teger, who is now at the Educational Testing Service, Princeton, New Jersey.

If drunk and sober subjects accepted the same level of risk in such a task, then Cohen's conclusion would be more clearly supported. In the present experiment it was hypothesized that they would not. That is, in line with common-sense assumptions, drunk subjects were expected to accept greater risk than sober subjects.

Research using alcohol frequently ignores differences among various beverages; yet alcoholic beverages which contain an equal amount of ethyl alcohol (as expressed by the proof) often differ in *congener* content (Snell, 1958). Congeners, such as fusel oils, are known to contribute to the color and taste of the alcohol and have recently been found to affect reaction time and performance on perceptual motor tasks (Katkin & Hayes, 1967) and visual nystagmus (Murphree, Price, & Greenberg, 1966).

Katkin and Hayes have also concluded:

> Congeners do have deleterious effects, and these effects are probably maximized when the subject has to engage in decision making rather than simple "reflexive" responding [1967, p. 10].

Therefore it was also hypothesized that in the present study congeners would produce greater risk taking, since risk taking is a characteristic of certain kinds of decisions. Thus, for example, subjects drunk on bourbon should take greater risk than those drunk on vodka, for bourbon has 100 times the congener content of vodka (Snell, 1964).[2]

Variables other than alcohol ingestion have been linked to increased risk taking. Wallach and Kogan (1961) have found that younger people take greater risks than older people. There is also considerable evidence that groups are riskier than individuals (Kogan & Wallach, 1967a). The study of different age groups and many of the studies comparing groups to individuals have used the same measure of risk taking, the choice-dilemma questionnaire devised by Wallach and Kogan (1961), which measures risk taking on a variety of topics. The present study used the choice-dilemma task also, in order that the effect of alcohol might be compared to the effects of age and group decision in regard to the topics on which risk taking is affected.

## METHOD

### *Subjects*

The subjects were 36 male graduate students at the State University of New York at Buffalo, all over 21 years of age. All were volunteers for an experiment involving the use of alcohol and were paid $20 each for their participation. Subjects were assigned randomly to the various conditions. One had to be dropped from the study for failure to follow instructions.

### *Measure of Risk Taking*

The decision task was Kogan and Wallach's (1964) 12-item choice-dilemma questionnaire. Each item requires the subject to give advice to a hypothetical person who is faced with a difficult decision. The subject must decide whether to advise the person to take a risky or a nonrisky alternative. If the risky alternative is chosen, he must also decide the minimum odds of success which should be demanded before choosing that alternative. The most risky response available involves choice of the risky alternative with $\frac{1}{10}$ chance of success. The subject might also recommend the choice of the risky alternative with 3, 5, 7, or 9 chances in 10 of success. The most cautious response would be the choice of the nonrisky alternative. In all items the risky alternative involves a better outcome *if it is successful*. If the risky alternative is *not* successful, then the nonrisky alternative would be preferable. The items deal with a wide range of topics, including health, sports, politics, career choice, and finance.

### *Procedure*

The subjects arrived at the laboratory early in the morning. They were weighed and their blood-alcohol percentage was checked by means of a Stephenson Breathalyzer to be certain that they had not been drinking. Subjects were run in groups of two, but were not permitted to talk during the testing sessions or to discuss the tests during the intervals between the testing sessions. Subjects were assigned to the following two groups.

*Group I*. The subjects were asked to fill out the choice-dilemma questionnaire. After they had finished, they were given their first dose of alcohol. Three more doses were given at hourly intervals. One-half hour after the fourth and final dose they were given the choice-dilemma questionnaire a second time. They were told that their first answers had been for practice, so that they could become familiar with the problems, and that they should reconsider each problem carefully.

*Group II*. The subjects were given four doses of alcohol at hourly intervals, identical to the subjects in Group I. One-half hour after the fourth and final dose they were given the choice-dilemma questionnaire for the first time. Two and one-half hours

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[2] Scotch, used by Cohen, lies midway between bourbon and vodka on the congener continuum.

TABLE 1

MEAN RISK IN EACH CONDITION

| Questionnaire administration | Type of alcohol | | | Average |
|---|---|---|---|---|
| | Bourbon | Vodka | Synthetic alcohol | |
| Group I | | | | |
| First administration, sober | 6.18 | 5.37 | 5.57 | 5.71 |
| Second administration, drunk | 5.90 | 5.23 | 5.25 | 5.42 |
| Group II | | | | |
| First administration, drunk | 4.12 | 5.58 | 5.16 | 4.93 |
| Second administration, sober | 4.18 | 5.79 | 5.23 | 5.13 |

Note.—The smaller the number, the greater the risk.

later, after their blood alcohol had dropped to .03% or lower, the choice-dilemma questionnaire was administered again, with the explanation that the first questionnaire had been for practice.

### *Alcohol Composition and Dosage*

Each dose of alcohol contained .2 milliliter of ethanol per kilogram body weight. Thus, each subject ingested a total of .8 milliliter ethanol per kilogram body weight, sufficient to get him mildly drunk. One-half hour after the fourth dose the mean level of blood alcohol was .08%. In order to obscure any taste differences among the various types of alcohol, crushed ice was held in the mouth for a few seconds immediately before drinking the alcohol.

The subjects were assigned randomly to one of three alcohol conditions:

1. *Bourbon.* Subjects received a synthetically produced beverage of 80-proof alcohol to which four times the normal amount of (synthetically produced) bourbon congeners had been added.
2. *Vodka.* Subjects received a synthetically produced beverage of 80-proof alcohol to which four times the normal amount of (synthetically produced) vodka congeners had been added. Even under these conditions the congener content of the beverage remained close to zero.
3. *Synthetic alcohol.* Subjects received 80-proof synthetic alcohol, essentially congener free.

The congener level of the synthetic bourbon and vodka was set at four times the level of the commercial beverage so that the effect of the congeners would be strong without an excessive dosage of alcohol. Neither the experimenter nor the subjects were aware of which beverage the subjects were drinking, but all subjects knew that they were getting synthetic alcohol.

## RESULTS

The results of this study fall into three categories: the effect of alcohol on risk, of congener content on risk, and of alcohol on the pattern of risk across problems.

### *Alcohol and Risk*

The mean risk in each group is listed in Table 1. It may be noted that subjects in both groups were riskier when they were drunk than when they were sober, regardless of beverage (Group I: $t = 6.33$, $df = 17$, $p < .001$; Group II: $t = 3.87$, $df = 16$, $p < .01$). Although these results support the first hypothesis, their value is questionable because the repeated-measures design on which they were based apparently caused many subjects to develop insight into the hypothesis. After receiving the questionnaire for the second time, quite a few stated that they believed the experimenter expected them to be riskier when they were drunk than when they were sober. (Some indicated that they would not comply with this "obvious" demand.) Because of the danger of artifacts resulting from this insight, a decision was made to base all analyses on the between-groups data obtained from only the *first* administration of the questionnaire. At the time of this administration the subjects in Group I were sober and the subjects in Group II were drunk.

The definitive test of the first hypothesis was therefore made by comparing Groups I and II on the first administration of the questionnaire. Again, as hypothesized, drunk subjects on the average took greater risk than sober ones ($F = 4.26$, $df = 1/33$, $p < .05$). This comparison does not, of course, completely eliminate potential demand characteristics, since in the first administration of the questionnaire in Group II the subjects were aware that they had been drinking alcohol in a psychological experiment. However, in designing the experiment, it did not seem desirable or possible to go further in eliminating demand characteristics, since in real life people who are drunk usually know that they have been drinking. Such awareness presumably contributes to the ordinary effect of alcohol on behavior and, hence, probably belongs in research on the psychological effects of alcohol.

### *Congeners and Risk*

The comparison between drunk and sober subjects on the first administration was investigated further by comparing all the sober subjects (i.e., the overall mean of Group I) with subjects drunk on each of the three kinds of alcoholic beverages in Group II. Only the sober-bourbon comparison was significant ($t = 2.78$, $df = 31$, $p < .05$), suggesting that the sober-drunk difference obtained when all drunk subjects were combined was primarily due to the effect of bourbon. The sober-vodka and sober–synthetic-alcohol comparisons were in the predicted direction but not statistically significant.

The congener effect was further studied by comparing the impact of the three alcohol conditions on answers to the first administration of the questionnaire in Group II. The subjects given bourbon showed significantly more risk than those given vodka ($q = 4.22$, $k = 3$, $df = 10$, $p < .05$, Newman-Keuls method; Winer, 1962) and tended, although not significantly, to show more risk than those given synthetic alcohol ($q = 2.28$, $k = 2$, $df = 9$, $p < .20$). The difference between subjects given vodka and those given synthetic alcohol was slight ($q = .99$) and not significant. The effects of the congeners cannot be attributed to demand characteristics, since the subjects did not know what type of alcohol they were drinking. It should be noted, however, that the previous results are based on a rather select set of comparisons among the various experimental conditions. It would therefore be wise to accept the results cautiously and to view the conclusions which follow in the Discussion section as working hypotheses rather than as substantiated facts.

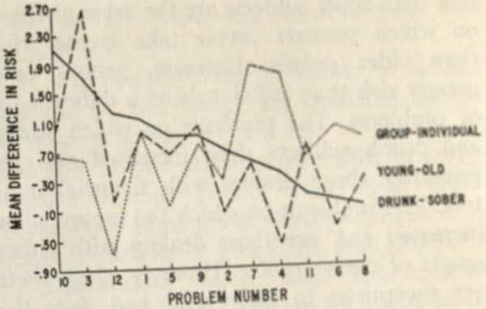

FIG. 1. Pattern of risk due to the effect of group decisions (Pruitt & Teger, 1967), age (Wallach & Kogan, 1961), and alcohol (present study). (The problems are arranged in order of decreasing differences between drunk and sober subjects. The 12 problems may be found in Kogan & Wallach, 1964, Appendix E.)

### *Pattern of Risk*

Although younger subjects are generally riskier than older subjects (Wallach & Kogan, 1961) and groups are generally riskier than individuals (Pruitt & Teger, 1967), the same problems do not account for the difference in each case. That is, group decision making leads to increased risk taking on one subset of problems from the questionnaire, while younger people are riskier than older people on a different subset. The *pattern* (i.e., the profile) of differences in risk over the 12 problems is shown in Figure 1 separately for the young-old and the group-individual comparisons.[3] That there is little relationship in pattern between these two comparisons is shown by an insignificant correlation over problems of .11 ($N = 12$).

The pattern of differences in risk taking between the drunk and sober individuals in the present study over the 12 problems is also shown in Figure 1. This pattern is similar to the pattern found in previous research for the young-old difference ($r = .63$, $N = 12$, $p < .05$) but not to that previously found for the group-individual difference ($r = -.26$, $N = 12$, *ns*). (Once again the drunk-sober data are based on the first administration of the questionnaire only.) Another way to phrase this finding would be that the problems on which drunk subjects take greater risk than sober subjects are the same as those on which younger people take greater risk than older people. However, groups take greater risk than individuals on a different set of problems. The problems on which young and drunk subjects show increased risk are primarily those dealing with financial matters. The problems on which the groups show increased risk are those dealing with either sports or career choice. However, since there are exceptions to these rules and since the number of problems in the questionnaire is rather small, it does not seem advisable to generalize such results to similar real life situations.

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[3] The group-individual data for this comparison is based on a summary of six studies as reported by Pruitt and Teger (1967).

## DISCUSSION

### *Risk and Alcohol*

The results indicate clearly that when people are drunk on high congener beverages (as typified by bourbon), they will take greater risk than when they are sober, even if the probabilities of success among which they must choose are clear and there is little opportunity to distort them. Trends in the data suggest that the same may also be true for low congener beverages (as typified by vodka and ethyl alcohol) and hence for alcoholic beverages as a whole; however, the latter conclusion is less secure. It is possible that the weakness of the data supporting the latter conclusion is due to the rather low level of alcohol used throughout the experiment (.8 milliliter ethanol per kilogram body weight). This level was chosen to enhance the effect of the congeners, since Katkin and Hayes (1967) had noted that congeners have the greatest impact on behavior at lower levels of alcohol.

These results can be integrated with those of Cohen, who used Scotch whisky, a beverage considerably higher in congener content than vodka. Cohen's findings suggest that drunk individuals (at least those drunk on congeneric beverages) distort the subjective probability of success, perceiving various alternatives as less risky than they actually are. The present findings suggest that when such distortion is not feasible they will demand a smaller objective probability of success before accepting the risky alternative. This may indicate that drunk subjects are either more concerned with the possibility of success or less concerned with the possible consequences of failure than are the sober subjects.

The use of a questionnaire measure of risk taking in the present study raises the issue of generalizability of results. This task involves hypothetical problems and as such produces no outcome for the subject as the result of his decision. However, this task has produced results parallel to those of more realistic tasks. The choice-dilemma questionnaire has been used to demonstrate increased risk in groups as opposed to individuals, a finding duplicated in a gambling task in which real money changed hands (Pruitt & Teger, 1969). High risk taking on this questionnaire task has also been correlated with high individual risk in a more traditional gambling task (Maehr & Videbeck, 1968). Although the choice-dilemma task appears to be a representative measure of risk taking, it would still be advisable to substantiate any results from it with a replication utilizing a task which produces real outcomes.

### *Risk and Congeners*

The results indicate that people drunk on high-congener beverages will take greater risk than those drunk on low-congener beverages. It will be recalled that Katkin and Hayes (1967) found that congeners produce impaired motor functioning. Hence, it can be concluded that congeners are partially responsible for the syndrome that is loosely called "drunkenness" by the general public. This conclusion may point to the need for modification of one of the methods currently used for measuring drunkenness, the blood-alcohol level, since the presence of congeners in the blood is not detected by this index.[4] However, the level of blood alcohol designated as an indication of drunkenness is usually high enough to override the effects of the congeners.

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[4] Cohen also criticizes the blood-alcohol test of drunkenness, though his arguments are not the same as those employed in the present study. He notes that the differences in risk within a group of subjects who have the same level of blood alcohol are far greater than the differences between groups with differing levels of blood alcohol. He attributes this to the possibility of different rates of digestion of alcohol and proposes that a psychological measure of risk taking be used as an index of drunkenness.

### *Pattern of Risk under Alcohol*

A high correlation was found between the pattern of risk associated with alcohol ingestion and that associated with younger age. This suggests that the same mechanism may account for the high risk taking of the drunk and the young. The results of several past studies make it possible to speculate about the nature of this mechanism. It may well involve reduced inhibition, which is manifested in lower fear of failure and hence an increased willingness to take risk.

The relationship between alcohol and inhibition has been discussed by Goodman and Gilman (1956):

> It may be stated categorically that alcohol is not a stimulant but a primary and continuous depressant of the nervous system. . . . The apparent stimulation results from the unrestrained activity of the lower centers freed by depression of higher, inhibitory control mechanisms [p. 98].

It seems reasonable to suppose that the reduced inhibition produced by alcohol frequently takes the form of reduced fear. Conger (1951) has noted that when rats in a conflict situation are given alcohol, they demonstrate a "decrease in avoidance response motivated by fear [p. 51]." Rettig (1966) has demonstrated that fear of loss is an important component of individual judgment in a risk situation.

Wallach and Kogan (1961) invoked the concept of inhibition in reference to age when they noted that in tasks involving cognitive functioning:

> the inferior performance of the older individual can be traced, in large part, to response inhibition, that is, an apparent preference for errors of omission rather than errors of commission [p. 25].

It is a short step from this assertion to the position that older people are inhibited by the fear of failure. It appears, therefore, that both alcohol and younger age involve a state of reduced inhibition associated with a reduced fear of failure. It is recognized that the effects of alcohol on cortical inhibition may not be equivalent to the reduced response inhibition in younger subjects; however, the common element of inhibition may prove to be a useful link between these two conditions.

If it is accepted that the effects of alcohol and age on risk taking are mediated by inhibition expressed in the fear of failure, it probably follows that the risky shift in group decision making results from *some other mechanism,* since group decision produces a different pattern or risk from that produced by alcohol and younger age. This conclusion may cast some doubt on the responsibility-diffusion theory of the risky shift (Kogan & Wallach, 1967b), which holds that individuals are initially inhibited from taking greater risks because they are afraid of the consequences of possible failure. In a group, however, they are able to spread the responsibility for their decisions to the other group members and thus reduce the fear of failure. Hence, this theory attributes the propensity of groups to take risks to reduce inhibition associated with diminished fear of failure. If this theory were true, one would probably expect to find a similar pattern of risk in group decision making as is produced by alcohol and younger age. But such is not the case.[5] It should be noted that the criticism just offered of diffusion of responsibility theory is based on the assumption that the pattern of increased risk across problems reflects the nature of the process that produces the overall increase in risk. Hence, the risk taking of drunk and youthful subjects is assumed to be unrelated to that of groups. This assumption may, of course, be wrong; that is, the pattern of increased risk across problems may be independent of the overall increase in risk. If so, this criticism of diffusion of responsibility theory cannot be maintained.

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[5] Evidence against the diffusion of responsibility theory has also been presented by Teger and Pruitt (1967) and Pruitt and Teger (1969).

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Dissonance and Relative versus Absolute Attractiveness of Decision Alternatives: Herbert J. Greenwald*: Communication Research Center, Boston University, 640 Commonwealth Avenue, Boston, Massachusetts 02215.

Personality Impression Formation as a Function of Relatedness of Information and Length of Set: Charles F. Schmidt*: Department of Psychology, Rutgers University, New Brunswick, New Jersey 08903.

Context Effects in Impression Formation: Robert S. Wyer, Jr.,* and Stanley F. Watson: Department of Psychology, University of Illinois at Chicago Circle, Box 4348, Chicago, Illinois 60680.

Types of Judges and Cue Utilization in Judgments of Intelligence: Nancy Wiggins,* Paul J. Hoffman, and Thomas Taber: Department of Psychology, University of Illinois, Urbana, Illionis 61801.

When is Aggression Funny?: Jonathan Dutman and Robert F. Priest*: Department of Psychology, University of Southern California, University Park, Los Angeles, California 90007.

Minimizing Differences in a Maximizing Difference Game: Gerald Marwell,* Kathryn Ratcliff, and David R. Schmitt: Department of Sociology, Social Science Building, University of Wisconsin, Madison, Wisconsin 53706.

Queue Counting: The Effect of Motives upon Estimates of Numbers in Waiting Lines: Leon Mann* and K. F. Taylor: Department of Social Relations, Harvard University, William James Hall, Cambridge, Massachusetts 02138.

Diastolic Blood Pressure Responses Following Direct and Displaced Aggression after Anger Arousal in High- and Low-Guilt Subjects: Salvatore Gambaro* and Albert I. Rabin: Mendota State Hospital, 301 Troy Avenue, Madison, Wisconsin 53704.

Effects of Aggressing "Alone" or "with Another" on Physiological and Psychological Arousal: John W. Baker, II,* and K. Sarner Schaie: Office of Military Psychology and Leadership, United States Military Academy, West Point, New York 10996.

Arousal Reduction via Self-Punitive Behavior: Lewis J. Stone and Jack E. Hokanson*: Department of Psychology, Florida State University, Tallahassee, Florida 32306.

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# SOCIAL PSYCHOLOGY OF A CLASSICAL CONDITIONING OF ATTITUDES EXPERIMENT[1]

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The Staats and Staats deception experiment for conditioning attitudes toward nonsense syllables is reexamined in the context of the social psychology of experiments. It is proposed that the "conditioning" effect was due to subjects becoming aware of demand characteristics and cooperating with them. Contrary to the results of Insko and Oakes, demand awareness is found to correlate better with the conditioned ratings than does contingency awareness. A direct test of learning of the associations between the visual and the spoken word lists also correlates highly with "conditioning" and the two awareness measures. The independent variable of psychological sophistication of subjects (time of semester) and a manipulation of the difficulty of making interlist associations are also significant as predicted. Results are interpreted as challenging a conditioning interpretation of the Staats' effect. The effect seems to be an artifact of the demand characteristics of the experimental situation.

Staats and Staats (1957, 1958) introduced a complex deception experiment which they claim results in the classical conditioning of an evaluative meaning or attitudinal response. Their subjects were told that they were participating in a learning experiment designed to study ability to learn two lists of words simultaneously. One list of six words was presented visually and was repeated many times. Immediately after each visual word, a word was spoken from another much longer list. Each of the spoken words was repeated only once. This procedure was intended to disguise the repeated contiguous association of spoken words having strong positive evaluative meaning (beautiful, healthy) with one of the visual words; and the association of strong negative evaluative meaning (ugly, sick) with another visual word. The other four visual words were included to disguise the associations and were always followed by spoken words of neutral evaluative meaning.

After "conditioning," subjects rated the six visual words on pleasant-unpleasant semantic differential scales. Instructions as to the purpose of the ratings were deceptive. Then subjects wrote down what they thought the purpose of the experiment was. Subjects verbalizing awareness of the relationship between the two lists were dropped from the analysis. The Staats' concern as to subjects' awareness of the association between the lists was a recognition that such awareness might invalidate a classical conditioning interpretation of the results.

Two studies (Cohen, 1964; Insko & Oakes, 1966) have subsequently challenged the Staats' conclusion that their result occurred "without awareness — without cognition [Staats & Staats, 1957]." The issue raised by these studies was in the context of the learning and contingency awareness controversy (Spielberger, 1962; Spielberger & DeNike, 1966). Cohen used the Staats' single-question awareness measure with a more stringent criterion for classifying a subject as unaware. Insko and Oakes used a more extended postexperimental questionnaire. Both studies found strong relationships between postexperimental assessment of awareness of the relation between the two lists (contingency awareness) and the so-called conditioned attitudes. These studies seem to challenge sufficiently the Staats' conclusion as to learning without contingency awareness. However, since neither study obtained evidence as to when the contingency awareness occurred, the correlations by themselves are consistent with two interpretations. Awareness could be causally linked to the marking of the rating scales, or the experimental effect (conditioning) could be ausing the postexperimental awareness. What s still needed is evidence as to which came irst, the awareness or the conditioning.

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Assuming for the moment that contingency awareness precedes the marking of the rating scales, and this can be tested by asking aware subjects to pinpoint the time during the experiment when they became aware, there is a broader context in which the attitude conditioning effect may be interpreted. That context is the social psychology of experiments (Orne, 1962; Page, 1968; Silverman, 1968). Contingency awareness may be only the first step in the complex sequence which brings about the "conditioned ratings." Perhaps the crucial factor in mediating the ratings is awareness of demand characteristics, or discovery of the purpose of the experiment, bringing about compliant or cooperative role behavior on the part of the subject (Page, 1968). That such factors are potentially very powerful mediators of behavior in human experiments is implicitly recognized by many experimenters (including the Staats') in that they often go to great lengths to disguise the true purpose of the experiment. In this context the important question with regard to attitude conditioning has to do with whether the deception was effective on all subjects; and if not did those who saw through the deception behave any differently than those who didn't (Kelman, 1967).

In the context of Orne's (1962) demand-characteristics formulation, there could be an alternative explanation of why the Staats' obtained their results. While their subjects were told to learn two separate lists of words, some of them did not follow directions; instead they learned or noted the consistent relation between pleasant and unpleasant spoken words and the visually presented syllables. For these contingency aware subjects the situation changed so that they no longer perceived themselves as participating in a rather dull and routine learning experiment. The experimenter had not fully explained; he must have something subtle and deceptive in mind. With this appraisal of the situation, they may have been set to search for other unexplained relations and to mistrust further instructions; they may even have conceived of the experiment as a test of their intelligence or problem-solving ability. With this or a similar set of expectancies, the aware subjects were presented the rating scales. At that point the purpose of the experiment became apparent to most of them; that is, "The experimenter wants to know if I caught on to the pleasant or unpleasant syllables and this is his way of testing," or "He was trying to condition my feelings," etc. In any case, some of the contingency aware subjects came to believe that the purpose of the experiment was for them to rate the crucial syllables in opposite directions and to the extremes of the scales. Having this knowledge of the purpose of the experiment (demand awareness) each subject then was faced with the problem of whether to cooperate with the experimenter and respond in the way that so obviously was demanded by the situation, or to resist the influence. In the majority of cases, the decision was made to cooperate (Page, 1968) and hence the "conditioned response."

Two previous studies (Cohen, 1964; Page, 1964) have found the distribution of subjects' ratings in attitude conditioning to be peculiarly bimodal. Subjects either rated the critical syllables as negatively or positively as possible or appeared, as a group, not to be affected. The difference between group means was produced by a few subjects conditioning a good deal rather than most of them conditioning a little. In another study (Page, 1968) concerned with awareness of demand characteristics in a deceptive learning situation, a similar bimodal distribution was found. The upper mode of the distribution in that study was populated entirely by subjects claiming to have been aware of and cooperating with demand characteristics. It may be that the bimodal distribution is typical of data mediated by demand characteristics. If so, the Staats' data is suspect. Also, not all subjects who verbalized contingency awareness in the previous studies showed the extreme conditioned rating. This suggests that something in addition to contingency awareness, possibly demand awareness plus motivation to cooperate, is necessary to produce the extreme ratings.

Insko and Oakes (1966) included measures of both contingency and demand awareness in their study. On the basis of their data they concluded that the concept of demand awareness does not add anything beyond simple contingency awareness. They go on to offer an explanation of the Staats' effect in terms of an aware concept formation rather than affective conditioning. Since their measure of demand awareness did not account for all the variance, they suggested that the Staats' phenomenon was not entirely an artifact of demand characteristics. In light of postexperimental interviews conducted earlier by the present author (Page, 1964) a "concept-formation only" interpretation seemed incomplete and a further investigation was considered worthwhile. For example, one female subject in that study said, "I don't really dislike the name Bill, that's my husband's name, but for purposes of this experiment, I marked Bill bad." Other subjects who were also extreme in their "conditioned attitudes" gave similar indications that their ratings were pure artifacts of demand characteristics.

Based on the above considerations, the present experiment was designed. Several predictions consistent with a demand-characteristics explanation were made. First, a strong association between carefully assessed postexperimental reports of having been contingency aware (knew the interlist association) during training and before marking the rating scales and the high conditioning scores was predicted. This is suggested by the Insko and Oakes correlations, but because they did not ask subjects to pinpoint the time at which they noted the contingency, they had no evidence as to direction of causation. Second, the awareness and conditioning association should be strengthened if demand awareness (knew the experimenter expected pleasant and unpleasant ratings) was separated from contingency awareness. This prediction is contradicted by Insko and Oakes, but in this study the questionnaire and scoring procedures were designed on the assumption that demand awareness is basically an either-or dichotomy; rather than attempting to measure awareness as a continuum as did Insko and Oakes. Third, a new "second learning test," which was a direct test of interlist associations given immediately after the rating scales, was predicted to correlate highly with the postexperimental awareness measures and the conditioned ratings.

In addition, two predictions were made with regard to independent variables. First, subject sophistication might significantly facilitate the conditioned rating behavior. This was a significant variable in a previous experiment (Page, 1968), and it is reasonable that subjects who have spent a semester listening to a psychologist lecture, reading a textbook, and participating in experiments (Holmes, 1967) would do better at figuring out what a psychologist might expect them to do in an experiment. This prediction is not necessarily required by the demand-characteristics formulation, but it is consistent with it. If evidence were found in support of this prediction, it would be difficult for the "conditioning" and the "concept-formation only" interpretations to account for.

The last hypothesis concerned a direct manipulation of the difficulty of learning the interlist associations. If this were possible, then it should reduce contingency awareness and consequently the probability of becoming aware of the correct demand characteristics, which in turn should reduce the conditioning. The rationale for this manipulation was similar to that of the color-naming manipulation of Insko and Oakes, although operationally the variables are quite different. For the Staats' interpretation of their experiment, since subjects supposedly didn't know the associations anyway and since the strength of response depends on number of pairings of the syllables with meaningful words, increasing the difficulty of noting interlist associations should have no effect on conditioning as long as number of trials are held constant.

## METHOD

### *Subjects*

Subjects were 288 introductory psychology students at the University of Nebraska at Lincoln. They were run in 12 groups of 24 each. Actually 19 other subjects were also run, but they were randomly eliminated, before the data were analyzed, to equalize the *N*s in the 12 groups.

### *Experimental Design*

For an overall description of the basic experimental design, the reader is referred to Staats and Staats (1957, 1958). The present independent variables were: psychological sophistication of subjects (naive versus sophisticated), difficulty of interlist association (2, 4, and 10 filler syllables), and direction of conditioning (reversal of the syllable-meaning association), making a 2 × 3 × 2 design. In addition, there were the assessed variables of interlist association (second learning test) and postexperimental measures of contingency and demand awareness.

Subject sophistication was varied by running half the subjects (naive) during the first 2 weeks of the university semester. The other half of the subjects (sophisticated) were run 11 weeks later in the semester. To control for possible biasing effects of early and late volunteers, subjects were not allowed to volunteer. The class rolls of three introductory sections were randomly divided and half were asked to participate early and the other half later. A plausible cover story was given for this procedure so as not to arouse suspicion, that is, that some learning experiments require random samples of subjects. Subjects run at the first of the semester were carefully sworn to secrecy until the end of the semester.

Association difficulty was manipulated by varying the number of neutrally paired or filler syllables included with the two crucial nonsense syllables. The crucial syllables were yof and wuh. In the easy condition ($AD_1$) only 2 filler syllables (laj and giw) were included with the crucial syllables. It was expected that with a list of only 4 nonsense syllables it would be rather easy to discover the relationship between the two lists. For the moderate difficulty condition ($AD_2$), 4 filler syllables were included (laj, giw, xeh, and qug). For the difficult condition ($AD_3$), 10 filler syllables were included (laj, giw, xeh, qug, meq, sij, vaf, vec, yim, and xad). It was expected that this amount of extra diversionary material would make the interlist association rather obscure. The effectiveness of this manipulation was checked by the "second learning test" of interlist association. Number of conditioning trials (18 pairings of pleasant or unpleasant words with yof and wuh) was held constant for all three groups.

### *Procedure*

Through the presentation of the semantic differential rating scales the procedure was essentially the same as that of Staats and Staats in their many conditioning experiments. Beyond that point the procedure was changed to accommodate the second learning test and the postexperimental questionnaire. The experiment was conducted in a large classroom and subjects were seated with at least one empty seat between them so as to discourage either talking or copying.

The same deceptive orienting instructions were read to all groups. These were similar, but not identical, to those of Staats and Staats (1957); that is, "This is an experiment to see how well we can learn two separate lists simultaneously through two different sensory modalities." The nonsense syllables were presented by means of a slide projector. Each syllable appeared for 4 seconds with a 1-second change time. The lists of syllables (4, 6, or 12 in length for the three difficulty groups) were repeated 18 times in unsystematic order. One second after a syllable appeared the experimenter gave a spoken word and subjects repeated it in unison. For the crucial syllables (yof or wuh) the spoken words were consistently pleasant for one and unpleasant for the other. The order was reversed for half of the groups. The remainder of the spoken words were of neutral connotation.

Following learning, subjects were given the same deceptive orientation used by Staats and Staats, that is, "How we learn lists of words may be affected by how we feel toward the various words." They then read instructions for marking semantic differential scales. The first learning test consisted of 24 nonsense syllables each on a separate page of a small booklet. Beside each syllable was a blank line and below it was a pleasant-unpleasant semantic differential scale. Subjects rated all 24 syllables and made a check mark on the blank line if the syllable was one they had learned.

To this point the procedure was essentially a replication of the Staats and Staats procedure with the exception of the manipulation of association difficulty and the extra syllables added to the rating booklet so as to make it less obvious. The second learning test, consisting of a sheet of paper containing six syllables with large blank spaces below them, was then introduced. Instructions were as follows:

> Now, I also want to find out how many of the spoken words you can remember. We are passing out a recall test of the spoken words. We want to know how many associations between the spoken words and the written syllables you learned. So, for each nonsense syllable listed, write below it as many spoken words as you remember going with it. You may guess. This is a timed test and you will have only 5 minutes.

This measure represents an indirect index of awareness of the interlist contingency with considerable face validity.[3] It also has the advantage of being presented in the form of a test within the context of the experiment itself, rather than being postexperimental. The rationale is this: If subjects learned or formed the concept "good and bad words go with certain syllables," then they should be able to demonstrate it by writing down many more good and bad associates to the correct syllables than subjects depending on rote recall of something they had not been instructed to learn.

Finally, the postexperimental written questionnaire was introduced with orienting instructions, like those used in a previous study (Page, 1968), to enlist the honesty and cooperation of subjects while releasing them from the demand characteristics accompanying the experiment proper. Since there are many forms of postexperimental questionnaires currently in use, and since the resolution of the contradiction between the data of Insko and Oakes (1966) and those of the present study may involve differences in questioning and scoring, the entire postexperimental interview is reported in Table 1. Each

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[3] The author is indebted to Katherine E. Baker for suggesting this measure.

TABLE 1

POSTEXPERIMENTAL QUESTIONNAIRE

1. What was the purpose of this experiment and what were you supposed to do?
2. During the experiment did you ever have the idea that its purpose might be something other than what I was telling you? What?
3. Thinking back to the experiment, did you notice at the time any relationship between certain syllables on the screen and the words that were spoken? What?
4. If you noticed any relationship between the lists, is this something you were actually aware of during the experiment or is it something you thought of while filling out these questions?
5. Do you remember approximately when it was that you noticed this? (1) right away, (2) first $\frac{1}{3}$ of learning, (3) second $\frac{1}{3}$, (4) last $\frac{1}{3}$, (5) while taking the first learning test, (6) while taking the second learning test.
6. What did you think was the purpose of the rating scales at the time you were filling them out, if anything?
7. How did you go about deciding what rating to give the various nonsense syllables?
8. Did you think that the experimenter might have expected that you would rate certain of the nonsense syllables in any certain way? Explain.
9. Was your answer to Question 8 something you were actually aware of before or during the marking of the rating scales, or something that you thought of afterwards?
10. What syllable was always or usually paired with travel words?
    a. How certain are you of this or are you guessing?
    Guessing——:——:——:——:——:——Certain
    b. Is this something you were aware of during the experiment or something you thought of since? Please explain if necessary.
11. What syllable was always or usually paired with words of pleasant meaning?
    a. How certain are you of this or are you guessing?
    Guessing——:——:——:——:——:——Certain
    b. Is this something you were aware of during the experiment or something you thought of since? Please explain if necessary.
12. What syllable was always or usually paired with words of unpleasant meaning?
    a. How certain are you of this or are you guessing?
    Guessing——:——:——:——:——:——Certain
    b. Is this something you were aware of during the experiment or something you thought of since? Please explain if necessary.
13. Were you ever aware during the experiment that yof [wuh for the other group] was always paired with words of pleasant meaning or connotation and that wuh [yof] was always paired with words of unpleasant meaning? And, if so, were you aware of any effect this might have had on you as you marked the rating scales? Explain.
14. Assuming that you knew the pleasant and unpleasant words and what was expected on the marking of the rating scales, rate your attitude while marking the rating scales.
    Resist the influence——:——:——:——:——:——:——Mark the right answers
15. Please make any other comments that you feel might help us understand your reaction to this experiment.
16. Have you had any previous courses in psychology such as in high school?
17. Do you know the meaning of the term conditioning? If so, did you think about it during this experiment?

question appeared on a separate sheet of a small booklet.

The assumption behind this questionnaire is that awareness, like problem solving, is basically a dichotomy. A subject either knows it or he doesn't. Also, subjects aren't likely to tell about such things as demand awareness unless specifically and carefully asked. The use of brief or vague questions may lead to either too many false positives or false negatives or both. These incorrectly scored subjects of either type would reduce any association that might actually be present. Questionnaires should have multiple indicators of awareness, and judges should score in terms of the total context of the questionnaire. Notice Questions 11 and 12, which are crucial questions for contingency awareness. Not only does a subject have to write down the correct contingency, but he has to be reasonably certain without guessing and willing to say he knew it earlier and isn't reflecting back now that the experiment is over. With this type of multiple-criteria approach, one should be able to separate most of the aware from the unaware subjects.

### *Scoring Procedures*

It was not possible to use the original Latin-square analysis (Staats & Staats, 1957) based on each subject having two scores. Because this study added two extra factors, it was more convenient to assign each subject a single conditioning score (yof rating minus wuh rating) which is equivalent to the previous scoring. This score can vary from $-6$ to $+6$ and should average approximately 0 for neutral nonsense syllables if there is no conditioning.

The second learning test of interlist associates was scored so as to maximize the measurement of learning of the correct concept (good words with yof and bad with wuh, or the reverse). Subjects who wrote down several guesses that were neither pleasant nor unpleasant had not learned the correct concept. Therefore, the most sensitive measure of interlist association would be total correct associates for the two crucial syllables minus total incorrect. Because this task proved to be impossible for subjects who hadn't formed the concept, this scoring resulted in a minus number (which depended on amount of guessing) for those who hadn't, and a large positive number for those who had. Actually, a cutoff score of 4 (2 for each syllable) was selected as a minimum criterion for considering a subject to have formed the concept.

The postexperimental questionnaires were read and scored by two independent judges. The judges scored for contingency awareness (did the subject say he was aware, during learning, of the interlist association) on a 4-point scale of (1) clearly aware, (2) probably aware, (3) probably unaware, and (4) clearly unaware. Most subjects fell in either Categories 1 or 4. Only questions from 1 to 12 were considered. The judges made an overall evaluation of the consistency and clarity of the subjects' responses to all questions, but keyed on Questions 11 and 12, one of which had to be answered correctly and with certainty for the subjects to be considered contingency aware. The judges' ratings correlated $r = .96$ and they disagreed as to the dichotomy aware-unaware on only 5 of 288 subjects.

The judges then scored for demand awareness (did the subject say he was aware, before marking the rating scales, of how the experimenter expected him to rate the syllables) on the same 4-point scale. Any subject who was not contingency aware could not logically be considered to have been aware of correct demand characteristics. This may be an important difference between the present measure and the Insko and Oakes measure of demand awareness, because in the present data several subjects knew that the experimenter expected something, but they didn't know what. Also, some contingency aware subjects reported that they realized what was expected too late for it to affect their ratings; these were not considered demand aware. The judges correlated $r = .91$ on their ratings of this variable. While this is a high reliability, the difference in reliability between contingency and demand awareness suggests that the latter was more difficult to judge from the protocols. There were 20 disagreements for aware-unaware for this variable. For both demand and contingency awareness the average of both judges' ratings was used as the index of awareness in the analysis. Cooperation-resistance was scored keying on Questions 13 and 14. Knowing about conditioning and thinking about it during the experiment were scored separately for Question 17.

TABLE 2

MEAN CONDITIONING (DISREGARDING DIRECTION OF CONDITIONING) FOR THE SIX GROUPS OF THE EXPERIMENT

| $S_s$ | Association difficulty | | | $M$ |
|---|---|---|---|---|
| | $AD_1$ | $AD_2$ | $AD_3$ | |
| Naive | 1.43 | 1.15 | .23 | .94 |
| Sophisticated | 2.11 | 2.21 | 1.08 | 1.80 |
| $M$ | 1.77 | 1.68 | .66 | 1.37 |

TABLE 3

SUMMARY OF THE ANALYSIS OF VARIANCE PERFORMED ON CONDITIONING SCORES FOR TWO LEVELS OF SOPHISTICATION, THREE LEVELS OF ASSOCIATION DIFFICULTY, AND TWO DIRECTIONS OF CONDITIONING

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Sophistication (A) | 1 | 24.50 | 2.74 |
| Difficulty (B) | 2 | 25.45 | 2.84 |
| Conditioning (C) | 1 | 539.01 | 60.22** |
| A × B | 2 | 4.03 | — |
| A × C | 1 | 53.39 | 5.97* |
| B × C | 2 | 36.70 | 4.10* |
| A × B × C | 2 | 1.94 | — |
| Error | 276 | 8.95 | |

* $p < .05$.
** $p < .001$.

## RESULTS

Results for the independent variables of subject sophistication and association difficulty are presented in Tables 2 and 3. The analysis of variance was performed on the signed conditioning scores described earlier. In Table 2 the data were collapsed over the two syllable-sign conditions so as to give a clearer picture of the differences between the two more important variables. In Table 2, conditioning is indicated by a positive deviation from zero, otherwise the scores used in Tables 2 and 3 are equivalent. The means in each of the cells of Table 2 are based on *N*s of 48.

In Table 3, notice first the strong conditioning effect ($F = 60.22$, $p < .001$) showing that a replication of the Staats' results was obtained. Because of the nature of the experimental design (half of the subjects conditioned in one direction and the other half in the other), the hypotheses regarding subject sophistication and association difficulty are tested by the interactions of these variables with the conditioning effect. In Table 3 the interaction between sophistication and conditioning is significant ($F = 5.97$, $p < .05$). Notice (see Table 2) that the sophisticated subjects showed more conditioning ($\bar{X} = 1.80$) than the naive subjects ($\bar{X} = .94$) as predicted. The interaction between association difficulty and conditioning is also significant ($F = 4.10$, $p < .05$). From Table 2 we see a marked attenuation of conditioning in the difficult condition ($\bar{X} = .66$) relative to the other two conditions ($\bar{X}$s $= 1.77$ and $1.68$) as predicted. Scores on the second learning test paralleled the pattern of results in Table 2, indicating that an attenuation of interlist association did occur in Group $AD_3$.

Recall the predictions concerning the relationships between the conditioned rating responses, the test for interlist association, and the measures of awareness. Since these variables were all either dichotomies or sharply bimodal, they were made into dichotomies before testing the association between them. Chi-squares were computed and converted into phi coefficients (see Table 4) to obtain a measure of strength of association. All of the relationships were very strong and highly significant as predicted. Under each phi coefficient is given the appropriate $\text{phi}_{\text{max}}$. This provides a basis for comparing strengths of association as $\text{phi}_{\text{max}}$ is the maximum value of phi obtainable with the given marginal proportions (Guilford, 1956).

The tight association between all of the variables in Table 4 is striking. This means that if a subject had a high conditioning score (+4 or greater difference between positive and negative syllable), there is a high probability that he also did well on the test of association. He also was able to state postexperimentally the interlist contingencies and claimed to have been rather certain of them during the learning and before the rating. He also claimed that he knew, before marking the rating scales, how the experimenter expected him to mark them. This is very strong support for the explanation of the Staats' results presented earlier.

While all the variables in Table 4 are tightly bound together, the small differences in strength of association are meaningful. The best predictor of conditioning is demand awareness, not contingency awareness as claimed by Insko and Oakes. In fact, 18 subjects were contingency aware who weren't demand aware, and these subjects did not show high conditioning. The mean for this group, when conditioning was scored as a positive deviation from 0, was .11, which was not significantly different from 0 ($t = .03$, *ns*). The mean of the unaware group was also almost exactly 0 ($-.02$). The mean for the subjects who were both contingency and demand aware was 4.6, which was quite significantly different from 0 ($t = 37.33$, $p < .0001$). While demand awareness is closely associated with contingency awareness (the first criterion for demand awareness was contingency awareness), the slight discrepancy between these measures makes demand awareness a stronger correlate of the conditioned ratings.

Figure 1 presents the distributions for conditioning scores of demand aware and cooperating versus unaware subjects. The direction of conditioning is folded over in this figure so that positively signed scores mean behavior in the direction of conditioning. Notice the symmetrical distribution of scores for unaware subjects; only two had conditioning scores of 6 while more than 50% of the demand awares had scores of 6. In Figure 2, notice that a distribution of "conditioning"

TABLE 4

SUMMARY OF PHI COEFFICIENTS (OVER THEIR APPROPRIATE PHIMAX VALUES) OF ASSOCIATIONS BETWEEN FOUR DICHOTOMIZED VARIABLES

| Variable | 2 | 3 | 4 |
|---|---|---|---|
| 1. Conditioned ratings | .67/.90 | .81/.93 | .69/1.00 |
| 2. Contingency awareness |  | .86/.88 | .79/.88 |
| 3. Demand awareness |  |  | .76/1.00 |
| 4. Association learning |  |  |  |

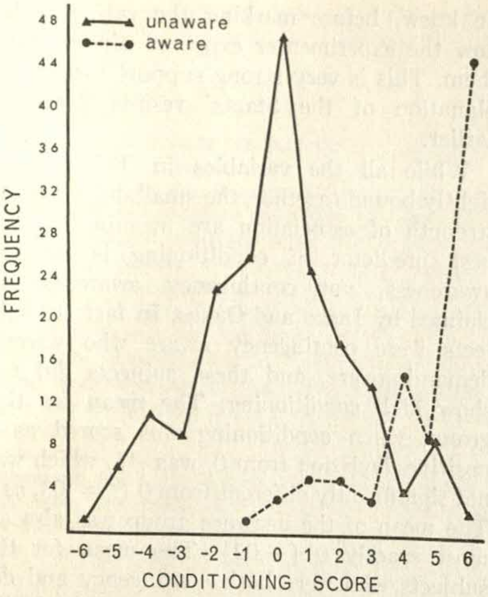

FIG. 1. Frequencies of conditioning scores on experimental syllables yof and wuh (positive minus negative syllable) for the unaware ($n = 195$) versus the demand aware and cooperating subjects ($n = 86$).

scores artificially generated on control syllables looks just like the distribution for unawares on experimental syllables. Only demand awares rated the experimental syllables any differently than control syllables.

What of the demand aware subjects with positive but not extreme scores? A few at 4 were aware of both contingencies, but did not use the ends of the scales. Most demand aware subjects with conditioning scores of 5 or less were only aware of one contingency. If a subject was aware that extreme ratings were expected for only one syllable and rated it accordingly while rating the other syllable according to his actual feelings about a nonsense syllable, then his conditioning score should be positive and would vary according to how he rated the syllable on which he was unaware. This is exactly what happened. All aware subjects with conditioning scores of 6 were aware of both contingencies; most with lower scores were aware of only one. These findings are very consistent with the general formulation which generated the experiment.

Cooperation-resistance has not been discussed in this paper because the number of resistors ($n = 7$) was much smaller than found in a previous study (Page, 1968). However, these few who reported resisting did not show conditioning, corresponding to the previous results.

Recall that subjects were asked whether they knew the meaning of conditioning and if they had thought about it during the experiment. The argument of this paper does not depend upon aware subjects knowing or thinking about conditioning; consequently no predictions were made, but it would be interesting support for the argument if some of them did. The association between sophistication and knowing the meaning of conditioning was highly significant ($\chi^2 = 70.6$, $df = 1$, $p < .001$). Only 10 of 144 sophisticated subjects did not know the meaning of conditioning while this was true for 75 of the naives. Most introductory students apparently do learn something during a semester. What is more interesting is that almost half of the naive group also knew about conditioning, and therefore "naive" wasn't an entirely correct description of this group. The association between knowing the meaning of conditioning and demand awareness was also significant

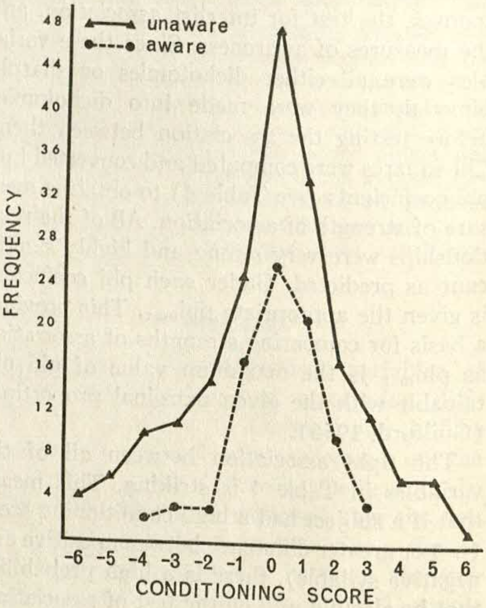

FIG. 2. Frequencies of conditioning scores on control syllables laj and giw (first minus second syllable with order reversed for half the subjects) for the unaware versus the demand aware and cooperating subjects.

($\chi^2 = 9.65, df = 1, p < .01$), though it doesn't account for a large amount of total variance. Of those ($n = 203$) who knew the meaning of conditioning there was a highly significant association ($\chi^2 = 36.55$, $df = 1$, $p < .001$) between thinking about conditioning during the experiment and demand awareness. Perhaps these data should not be overinterpreted, but they seem to suggest that while knowing about conditioning (and probably other psychology-related concepts) and thinking about it are not required for becoming demand aware in this situation, they do facilitate it.

## DISCUSSION

Every hypothesis of this study was clearly supported and all the converging evidence seems to attest to the essential correctness of the formulation which generated the predictions. It appears that the Staats' (1958) classical conditioning of evaluative affect or attitudes interpretation of their data is incorrect. The reaction they evoked in their subjects was far too complex, and apparently cognitively mediated, to be labeled classical conditioning. This critique says nothing about the general theory which they believe was demonstrated by their experiment (Staats, 1967). Attitudes certainly are learned and perhaps sometimes through a process similar to classical conditioning. But this neat little deception experiment, using college sophomores as subjects, does not seem to demonstrate it.

The present study supports the idea that the so-called conditioned attitudes are entirely artifacts of demand characteristics. This is not to say that the social perception, social influence, and other social psychological variables present in the total laboratory situation were not genuine and interesting psychological phenomena, but they are artifactual sources of variance from the point of view of what the original experiments were designed to study. Perhaps the demand-characteristics problem is such a limiting factor on deception experiments that the kinds of hypotheses the Staats' were interested in testing simply cannot be tested in the psychological laboratory at the present stage of development.

From the present vantage point it seems that while attitudes are learned phenomena, it is rather naive to suppose that a brief, deceptive, and highly artificial laboratory experiment could induce genuine attitudes towards neutral stimuli. Perhaps psychologists would do better in the future to recognize the limitations of "quick and easy" deception experiments such as here described. The popularity of such methodology in our discipline seems symptomatic of a kind of worship of experimental manipulations for their own sake (Bakan, 1967) without due regard for the complexities or the actualities of the social context in which these manipulations occur (Sherif, Sherif, & Nebergall, 1965).

The conflict between the present results and those of Insko and Oakes (1966) as to the relative importance of contingency and demand awareness remains to be clarified by further empirical investigation. While they found that demand awareness could not account for all of the variance (Insko, 1967, p. 29), the present study suggests that it can. There are many procedural differences between these two studies; most specifically the different questionnaires and scoring techniques used in assessing awareness. In addition to differences in specific operations there were basic differences in assumptions about the nature of so-called "awareness." In the present study an elaborate and multifaceted written questionnaire was used to answer a simple question: Did the subjects know the approximate purpose of the experiment while it was going on? Insko and Oakes used a simpler oral questionnaire to answer a more difficult question. Under the assumption that demand awareness is a continuum, they attempted to quantify that continuum. They assumed that individual differences in clarity of expression in responding to questions was an accurate reflection of the so-called "continuum of demand awareness," and then classified degrees of clarity on an ordinal scale. If in fact demand awareness is more accurately thought of as a dichotomy, and Insko and Oakes were actually quantifying something only roughly correlated with awareness, then this could account for the lower correlation between their demand-awareness scale and the rating scales. However, which of these sets of assumptions about the nature of demand awareness is more adequate remains a question for further investigation.

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## ERRATUM

In the article by John T. Lanzetta and James M. Driscoll in the December issue, the institutional connection for John T. Lanzetta on page 479 should read: *Dartmouth College.*

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# EXPERIMENTAL DEMAND CHARACTERISTICS AND THE CLASSICAL CONDITIONING OF ATTITUDES

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As previously indicated, the issue of awareness versus conditioning in the context of learning experiments is the type of conflict in interpretation which arises in a science (Finley & Staats, 1967). That is, it is not unusual for the same experimental results to be interpreted in two different ways, in the context of two different theories. Sometimes such controversies are resolved by conducting a "crucial" experiment which clearly verifies one theory and refutes the other. Certainly, this is one of the dreams of many scientists.

More usually, however, the issue is resolved in other ways. One theory is found to have greater advantages in that its principles are extended to a greater number of events; the theory presents greater possibility for prediction and control of events; and thus it thereby generates more successful experimentation, and so on. It has been the author's plan to concentrate his efforts in the realm of developing the general goodness of a competitive theory rather than in criticizing competitors. That is, the author's emphasis has been on the development and extension of his learning theory in a comprehensive experimental and theoretical manner to various aspects of human behavior (see Staats, 1963, 1964, 1968a; 1968b; in press).

However, it is felt that it would be unfortunate if aspects of the learning theory and experimental methods were to be weakened because a criticism was successful through the absence of rebuttal. This brief note is thus being written in response to the interpretation Page (1969) has made of his experimental results and, in so doing, to other suggestions (see Cohen, 1964) that the classical conditioning of meaning may be attributed to "awareness" in the subjects, rather than to the conditioning process.

Thus, it seems worthwhile to indicate that Page's points are not crucial challenges to the author's learning theory of attitudes (see Staats, 1968a; 1968b), or to the experimental methods of the author which have been employed in a series of studies by the author and associates, as well as by other investigators. In beginning, it should be indicated that Page's results are not contested—the study appears to be very well conducted. It is the interpretation of the results that is in question.

Page says that a bimodal distribution of scores is the giveaway. In such cases of bimodal distributions he suggests that one must suspect that conditioning is not involved, but rather the demand characteristics of the procedures get the subjects to respond in a way that looks as if conditioning had occurred. The view that a bimodal distribution is inconsistent with the operation of learning principles in the present experimental procedure arises because Page did not consider the learning task in detail in learning terms. A bimodal distribution is not anomalous. While the principle involved is that of classical conditioning, the experimental situation is actually a complex one.

One must distinguish between the operation of the simple principle of classical conditioning and the complex behaviors necessary to get the conditioning. The conditioning occurs when the subject looks at the CS syllable and listens to the UCS word which elicits the attitude response to be conditioned to the CS. Simple as this may be diagrammed, in the actual experimental procedure some rather complex behaviors are required of the subject for the CS-UCS pairings to occur as they are supposed to. The subject must look at the CS and listen to the UCS. If he does not attend to both, conditioning will not occur. Some subjects do not attend; the language conditioning procedure is repetitious, boring, and seemingly fairly senseless. With six syllables paired each with 18 UCS words there are

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108 presentations in the conditioning phase. Some subjects do not attend, daydream, and so on.

Moreover, some subjects also employ various strategies for attempting to memorize the CS syllables together as a word-association chain. Or, the subject may attempt this with the UCS words, although the author developed the instructions to prevent these interfering behaviors. Thus, although the presentation of a CS syllable and UCS word may be considered to be a classical conditioning trial, it is not possible to strap the human subject into a harness where the stimuli may be reliably presented and the organism will not engage in the interfering activities. The author has been confronted with cases of other investigators who did not get the attitude conditioning because of deficient analysis of the experimental task, and consequent deficient procedures.

At any rate, it would be expected that there would be subjects who attend well and are highly conditioned, others who attend partially and are partially conditioned, and others who do not attend, or participate in interfering mnemonic behaviors, and who do not evidence conditioning. That is, the subjects who attend well and do not engage in mnemonic behaviors will receive the clearest cut conditioning. The bimodality characteristics of the conditioning procedures are determined in part by the percentage of subjects who are in the first category. It would be expected that differences in instructions and procedures (individual versus group conditioning, the manner in which UCS words are presented, and so on) would affect the extent and distribution of conditioning and awareness. For example, the author has observed that if the positive attitudinal words are pronounced in a positive manner and the negative words in a negative manner, there will be much greater conditioning and awareness—a sharper bimodality. It is interesting to note that 8½% of the subjects scored extreme conditioning in the author's original attitude (evaluative meaning) conditioning procedure employing nonsense syllables. In the latest study with which the author is acquainted,[2] the same procedures were followed except that the order of the CS and UCS was reversed in a backward-conditioning method. With this method conditioning was significant, but less, and there were only 7% of the subjects who had maximal scores. Page's results, it may be noted, show about twice this percentage maximally conditioned, perhaps due to procedural variables which would enhance extreme conditioning and thus awareness. At any rate, the bimodality of the distribution is no evidence for or against a conditioning interpretation. Bimodality arises because of the extraconditioning behaviors the subjects display in the experiment, and the other variables mentioned.

Page also makes a point of finding a relationship between what he calls the naiveté-sophistication variable and the conditioning. The variable here is between early in the semester versus late in the semester participation—ample opportunity for the students to be informed about the experiment from other students or from reading about the experiment itself if certain introductory psychology textbooks are employed. This result is thus really not consequential as a verification of either the cognitive or conditioning interpretation.

Page also finds that recall of the CS and UCS associations depends on the "difficulty" of the task (number of irrelevant nonsense syllables in the task), and recall is related to the extent of conditioning and awareness. None of these findings contradicts the present author's conditioning theory, however, as was assumed. That is, Page's addition of nonsense syllables increases the repetitive, boring nature of the task, not its difficulty. (Page's interpretation of the difficulty variable is actually based upon an implicit assumption that the subjects are really attempting to divine the actual purpose of the experimenter.) This increase means that fewer of the subjects will continue to attend well, and fewer will be conditioned. Since fewer will experience the CS and UCS pairings, fewer will be able to indicate later what UCS words went with what CS. Let us assume, as will be elaborated below, that extent of awareness depends upon extent of conditioning. In that case, Page's manipulation of "difficulty" will produce fewer conditioned subjects and thus fewer aware subjects. As the learning analysis of the task indicates, the "difficulty" variable in Page's experiment would be expected to produce just the results obtained.

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At this point, it appears germane to challenge more directly the whole rationale of the demand-characteristic procedures employed, and thus the demand-characteristic position. That is, Page suggests that the instructions and conditioning task the present author developed actually "demand" from the subject that he rate the syllables in a certain way irrespective of a conditioning process. This demand characteristic is thought to function even though the instructions, and so on, were composed to be deceptive. It may be suggested, in contrast, that Page's questionnaire has far greater "demand characteristics" than the original procedures. (This is also true of the study by Insko & Oakes, 1966, which Page cites in his article.) The questionnaire actually *demands* that the subject indicate that he saw through the purpose of the experiment. This demand confronts the subject from the beginning item, with no deception involved. The item states: "What was the purpose of this experiment and what were you supposed to do?" From here on the subject is exposed item by item (if the subjects encountered the items sequentially) to additional stimuli that would effectively program him to the desired position—if he has made some of the necessary observations and can recall them.

The demand-characteristic rationale also assumes that the subjects are in some manner reticent about revealing that they know the purpose of the experiment and what they were "supposed" to do in response to the question the author originally posed to them. This item was: "Would you write down anything you thought about the experiment, especially anything you thought about the purpose of the experiment *while* you were participating in the experiment." The author still feels that this item pretty effectively elicits the subjects' thoughts about the experiment while they were participating. If anything, it probably elicits *more* awareness than that which occurred during the conditioning procedure. That is, rating the pleasantness or unpleasantness of the CS word probably also leads some otherwise unaware subjects to become aware when they later encounter the question concerning purpose.

In this context it is interesting to note that Cohen (1964), whom Page refers to, altered the original item to test awareness of the subjects—although this point was not clearly brought out in his study. That is, he changed the item by deleting the phrase "while you were participating in the experiment." Cohen obtained a greater number of aware subjects than in the original study as would be expected from the procedural difference. This should have been taken into account in his interpretation.

In any event, the variation of Cohen's results from the original results indicates how the questions asked after the experiment will influence the subjects' responses. On this basis, one has to challenge the questionnaire procedure itself. Depending upon how it is conducted, it will *produce* varying levels of "awareness" as well as measure it. The extent of influence on awareness is difficult to judge.

In his introduction, Page characterizes the process he feels occurs in the present author's attitude-conditioning procedures. That is, he suggests that the subjects become aware of the demands of the situation and respond accordingly. A contrasting view may be presented. This may be done by describing the response of one of the subjects to the question concerning the purpose of the experiment. She said that as she was participating in the study, she was thinking that she did not like the name Dutch (a CS word), and that at this moment the name flashed on the screen and was followed by a negative attitudinal word. The subject stated that she then realized what had been occurring and why she did not like the name Dutch. It seems quite likely that subjects could be conditioned to "awareness" in this manner and yet be led later to respond to a questionnaire in a manner that would corroborate the demand-characteristic interpretation.

At any rate, it is suggested that a usual subject who had been conditioned to a positive emotional response to a syllable, and who had learned some of the UCS words paired with the syllables could on the questionnaire come to be scored as an aware subject, even though he might not have been aware in any causative sense. Actually, an analysis which considered the conditioned attitude (emotional response) capable of mediating similar-attitude word responses would not even have to assume the subject had *learned* any syllable word pairs, even though he could cite them when questioned. Along this line, it is interesting that Pollio (1963) found that the words given as associates of the CS syllables had in many cases never been presented to the subjects in the conditioning procedures. However, even in those cases these word associates tended to be of the correct attitudinal meaning. Pollio concluded that the conditioned attitude mediated the occurrence of these word responses.

The preceding discussion suggests that we have a circumstance in which the same empirical events are interpreted differently within the two theoretical contexts. It is thus also relevant in presenting the learning theory to summarize some of the supporting evidence which has not been included in Page's paper. There are several experimental findings that should be brought to bear upon the matter.

First, the classical conditioning of attitudes in the first-order situation is well documented. A CS word paired with an aversive UCS will come to be rated as unpleasant, and also to be responded to emotionally as indexed by physiological measures of emotional response such as the GSR (Staats, Staats, & Crawford, 1962). Such emotional responses could not be considered as voluntary responses to the demand characteristics of the experiment, it should be noted. In this experiment the subjects after the conditioning also rated the CS word on an attitude scale (again, a 7-point pleasant-unpleasant scale). The subjects who had been in the conditioning procedure rated the word as significantly more negative than did control subjects. This rating, it should be recalled, would be interpreted by Page to be a result of the demand characteristics of the experimental procedure. Thus, it is important to see also that the intensity of the involuntary emotional conditioning was significantly correlated with the intensity of the attitudinal conditioning as measured by the rating scale. Subjects who were more intensely conditioned to an emotional response to the stimulus gave a more intense attitudinal response (rating) to the stimulus.

It may be added that Maltzman, Raskin, Gould, and Johnson (1965) have replicated these findings using the same type of procedure. It is interesting to note that in their study the intensity of the conditioning was varied over three groups of subjects by varying the intensity of the aversive UCS. The important point here is that the subjects could not be demand aware, or aware in any way, that they were receiving different intensities of conditioning, yet they were conditioned physiologically and by attitude rating in accord with the intensity of the conditioning. (The present author has also verified the first-order conditioning of attitudinal responses to words in both animals and preverbal children [Staats, 1968a]. Moreover, much of the literature on counterconditioning in behavior therapy is a verification of the classical conditioning theory of attitudes as indicated in Staats, in press.)

Results relevant to the awareness versus conditioning positions are also available from the higher-order attitude-conditioning studies, where attitude words are used as the UCS. For example, Pecjak and Smith (in press) have employed a modification of the author's original procedure in an interesting study. Two types of conditioned stimulus were employed, color stimuli or geometric forms. Four of each kind were used so there were eight conditioned stimuli. These stimuli were each paired with either positive, negative, or neutral attitudinal stimuli for each subject; two with positive attitudinal words, two with negative, and four with neutral words. The purpose of the experiment was to condition attitudinal responses to the separate color and form stimuli and then to combine pairs of colors and forms into compound stimuli and then measure the type of attitude response made to the compounds. The various combinations of compounds were made and extent of conditioned positive or negative attitudinal responses was tested. The results expected from the classical conditioning attitude theory occurred. Two positively conditioned stimuli when combined elicited the most positive attitude responses, the positive-neutral compound elicited the next most positive attitude responses, the neutral-neutral compound was neutral, the neutral-negative compound elicited a mildly negative attitude, and the negative-negative compound stimulus elicited a more intense conditioned negative attitude response. It is difficult to conclude that the subjects would have been *aware* of the complex conditioning they were subjected to, the complex combinations of conditioned stimuli with which they were presented, and the summation of conditioned attitudes which a demand-awareness interpretation would suggest they were "supposed" to display when the separate conditioned stimuli were compounded.

One other experiment will be cited in which the classical conditioning of attitudes occurred where the influence of demand characteristics can be negated. Yavuz and Bousfield (1963) conducted a study in which subjects had the deceptive task of learning English response words to Turkish stimulus words. Each English word was supposed to be the equivalent of the Turkish word with which it was paired. Criterion performance was three correct recalls of each of the English words upon presentation of the Turkish stimulus words. Actually, the putative English equivalents had been chosen to be either positive, neutral, or negative attitude words and thus to serve as potential UCS words, in the present terminology. The Turkish words may be considered to be the CS words which should come to elicit the attitude response elicited by the English word with which the Turkish word was paired. One week after this learning procedure the subjects were tested for knowledge of the "translations," and they also had to rate the Turkish words on a good-bad attitude scale. The rating was done *whether or not the subject could remember the English word he had learned in response to the Turkish word.* As expected on the basis of conditioning theory, the subjects had been conditioned to respond to the Turkish word with the attitude elicited by the English word that had been paired with the Turkish word. The important thing, however, is that *the subjects showed the attitude conditioning even when they could not remember the English word that had been paired with the Turkish word.* By the criterion which is set by cognitive psychologists, including Page, the subjects were not aware—but they were conditioned to the attitude nevertheless. In terms of the social psychology of psychological experiments, it is interesting to note that Bousfield's theoretical orientation was one which would have expected the conditioned attitude to depend upon recall of the English word, not upon the classical conditioning of attitudes.

Other related experiments which support the classical conditioning theory of attitude formation and change could also be cited here (for example, Lott & Lott, 1960; Razran, 1938, 1940; Schutz & Naumoff, 1964). A very interesting experiment has also been published by Early (1968). In this study, the present author's attitude-conditioning procedures were employed with fourth- and fifth-grade children. After positive attitudinal conditioning to the *names* of "isolate" children, these isolates were themselves approached more in the free play situation by the conditioned children than were control isolate children. This study begins to show that classically conditioned attitudes affect behaviors other than the behavior of scoring a rating scale—a very important demonstration.

There is a point, related to the last one, which is germane here. That is, according to the author's general learning theory of attitudes a stimulus which is a CS for an attitude response should also have other learning functions (see Staats, 1964, 1968a, 1968b; in press). For one thing, a stimulus which elicited a positive attitude should function as a positive reinforcer; one eliciting a negative attitude should function as a negative reinforcer (or punishment). Thus, the words used which elicited attitude responses in the classical conditioning procedures should also serve as reinforcing stimuli in an instrumental conditioning situation.

Finley and Staats (1967) have shown this to be the case with sixth-grade children. Using the same words that were shown to elicit positive attitudes in the experiments under discussion, it was found that a motor response could be learned by the children if the words were employed as reinforcing stimuli. When a positive-attitude word like VACATION, JOY, FAMILY, SUNSHINE, was presented after the motor response for one group of subjects the response increased in frequency. For another group, negative attitudinal words such as HURT, POISON, BITTER, THIEF, on the other hand acted as punishments and weakened the response. Neutral words had an intermediate effect. This study begins to explore the vast power that attitude stimuli have in molding our instrumental behaviors—an effect that has long been recognized in social psychology, albeit not within the context of explicitly stated empirical principles of behavior. The learning theory thus yields this expectation, and many others, which the cognitive position would not suggest.

This study is supplemented by another recent experiment (Pihl & Greenspoon, in press). That is, a word was paired, for different groups of subjects, with UCSs which had varying levels of positive attitude. The CS word for the different subjects should come to elicit different levels of positive attitude and this should be shown by the extent to which the word would reinforce an instrumental response. This expectation of the present *learning* attitude theory was supported. It is difficult to see how "demand characteristics" could have affected the extent to which the subjects were later instrumentally conditioned, as a function of the intensity of the original classical conditioning of the attitude response.

This summary by no means exhausts the studies supporting the classical conditioning analysis of attitude formation, in contrast to the "demand aware" interpretation. It may be suggested, however, that the evidence is very substantial and much of it has been influenced by the original experiments.

In conclusion, it may be noted that criticism of methodology may be very productive. The criticism may lead to improvements in the methodology developed in the substantive study. The criticism may, however, have its drawbacks also in cases where it dissuades individuals from accepting a productive theory or a productive experimental procedure. The present note is written to prevent the unjustified weakening of the learning theory of attitudes which in its elaborated form (see Staats, 1968b; in press) extends to a wide variety of human behavior in a manner important to the theorist, the experimentalist, and the practicing psychologist.

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# SALIENCE OF LOGICAL RELATIONSHIPS AMONG BELIEFS AS A FACTOR IN PERSUASION

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An experiment was conducted to test the predictions that increasing the salience of the logical relationships among beliefs makes any specific belief in the system more resistant to persuasion, but facilitates change on those beliefs logically related, but unmentioned, in the persuasive message relative to change in the belief attacked. Two manipulations, form of pretest and cognitive tuning, were designed to vary the degree of salience and integration of beliefs. The results were complex. Form of pretest affected the belief attacked in the manner predicted with subjects in the high-salience condition changing less; however, it had no effect on the logically related, but unmentioned beliefs. The cognitive tuning effects were such that subjects in the high-integration condition changed more on both the specific target of the message (contrary to prediction) and the unmentioned, but logically related, beliefs. These and other findings are discussed in terms of an organizing principle of consistency and its implications for the indirect effects of persuasion.

Research on the logical relationships among beliefs, using McGuire's (1960a, 1960b) syllogistic model, has consistently demonstrated an indirect persuasive impact on the logically related, but unmentioned, conclusion of a syllogism from a message directed at the minor premise (Dillehay, Insko, & Smith, 1966; McGuire, 1960b, 1960c). Another interesting finding of McGuire's original study was a Socratic persuasive effect whereby subjects changed toward greater consistency between the first and second presentation of a questionnaire in the absence of any persuasive message; however, Dillehay et al. (1966) were unable to replicate this finding in either of two studies. It is possible that this discrepancy is due to the more intelligent and highly educated subjects in the Dillehay et al. (1966) study, although it is not readily apparent why more intelligent subjects should show less Socratic effect unless they were at such a high initial level of consistency as to preclude significant additional change.

McGuire (1960c) suggested that experimental manipulations of the salience of logical relationships among beliefs would increase the logical repercussions of a persuasive message by disrupting the equilibrium between wishes and reality that appears to characterize a person's cognitive system at any given time and move it further into the logical, reality-oriented realm. Indeed, McGuire (1960c) raised the possibility that the logical repercussions might be due, at least in part, to the use of a before-after design in the study which may have heightened the salience of the interrelatedness of the syllogistic components, although Dillehay et al. (1966) subsequently obtained significant indirect persuasive effects while using an after-only experimental design.

It was the intention of the present experiment to manipulate systematically the salience of the logical relationships among beliefs in order to examine the effects upon persuasion as well as to test for the possibility that a Socratic effect occurs at certain ability levels, but not others, by using a heterogeneous sample of subjects.

It would seem clear that the more salient the relationships (or higher the degree of cognitive integration about the particular issue), the more resistant a subject would be to persuasion directed at a specific belief in the system. That is, when the beliefs are in a greater degree of interdependence, any change in a given belief demands some degree of corresponding change in the interrelated, but unmentioned, beliefs. Making these relationships more salient is somewhat analogous to bolstering a belief with additional consonant cognitions in order to confer resistance to persuasion (e.g., McGuire, 1964; Tannenbaum, Macaulay, & Norris, 1966) in that it reduces the possibility of a high degree of compartmentalization among the beliefs. Hence, an attack upon a specific belief introduces a greater degree of imbalance in the cognitive system. From this reasoning it follows directly that whereas increased salience of logical relationships among beliefs should inhibit change in the specific beliefs attacked, it should facilitate greater change in the logically related, but unmentioned, beliefs relative to the change in the specific belief. That is, in order to maintain consistency among beliefs, change in one demands some degree of corresponding change in interrelated others, and this tendency should be greater when the interrelationships are more salient.

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[2] Thanks are extended to William J. McGuire and M. Brewster Smith for their helpful comments on an earlier version of this manuscript.

Increasing the salience of the interrelationships among beliefs should also influence the Socratic effect, but the direction is difficult to predict. As McGuire (1960b) pointed out, heightening the subject's awareness of his inconsistency might either result in greater changes toward consistency or it might arouse the individual's hostility or defensiveness and, thus, have the opposite effect.

One manipulation of salience was accomplished by systematically varying the format of the pretest. In one form, the belief statements comprising the major and minor premises and the conclusions were presented in syllogistic order, whereas in the other form, they were presented in random order as McGuire (1960a) and Dillehay et al. (1966) had done. It would seem evident that the order of presentation on the pretest should vary the salience of the logical relationship between the minor premise and the conclusion.

A second manipulation designed to make the subject's cognitions about the topics more or less integrated was based on Zajonc's (1960) technique. At the time that the subjects received the persuasive messages directed against the minor premises of the syllogisms, half (chosen at random) were instructed that they subsequently would be called upon to communicate the ideas contained in the messages to others. The other half were told that they would listen to additional information but that to do so effectively, they must first fully comprehend the ideas contained in the messages. Zajonc (1960) demonstrated that such "tuning" instructions effectively altered the subjects' degree of cognitive organization about the topic in question. That is, when they thought they were to communicate to others, their cognitions were more highly integrated, or interdependent, than when they expected to listen to others. Other investigators using this technique have obtained predictable effects mostly in impression formation in that communicators formed more homogeneous impressions when presented with inconsistent information (e.g., Cohen, 1961) and devoted more attention to the side they were to support (e.g., Brock & Fromkin, 1968) than did the receivers. It seemed possible that these instructions might affect the subjects' learning of the persuasive material, with persons expecting to communicate to others learning it more thoroughly, thus confounding the results. To check on this possibility, subjects were given a demanding multiple-choice test of comprehension of the material.

Finally, standard ability scores were obtained on all subjects from their cumulative record files to allow a test for the Socratic effect at both high- and low-ability levels.

## METHOD

Fifty-three subjects, drawn from three introductory psychology classes at Diablo Valley Junior College, participated in both sessions of the study during their regular class meetings. The number of males and females was divided about evenly. During the first session all subjects rated 24 propositions, comprising eight of the syllogisms used by McGuire (1960b, 1960c), according to their estimate of the probability that a given statement was true. A 100-point scale ranging from 0—very improbable to 100—very probable was provided immediately below each proposition. Two forms of the pretest were distributed randomly among subjects. In one of these forms the propositions were presented as intact syllogisms, while in the other the propositions were distributed randomly throughout the questionnaire, with the restriction that any two related propositions were separated by a minimum of three unrelated statements. The experiment was represented to the subjects as a study of the relationship between the controversiality of subject matter and information processing.

Two days later all subjects read persuasive messages averaging about 250 words in length directed at the minor premises of four of the eight syllogisms upon which they had been pretested. The particular combination of four messages that any given subject received was randomly determined. Hence, each person served as an experimental subject on four of the eight syllogisms and as a no-message control on the other four. These eight communications, with some modifications, paralleled those of McGuire (1960b, 1960c). The "tuning" manipulation was implemented at this point through written instructions accompanying the persuasive communications with the two conditions being randomly assigned to the subjects. One group of subjects (communicators) was told that they would subsequently be called upon to communicate verbally the ideas expressed in the four messages. The second group of subjects (receivers) was informed that they would later listen to additional information related to the messages they were about to read but that to do so effectively, they must first fully comprehend the ideas contained in the messages. The latter part of the instructions was an attempt to compensate for possible differential attention to the messages between the two groups.

After the subjects had read the four messages, the signed materials were collected. Then all subjects again rated the probability of truth of the 24 propositions. On this occasion, the propositions were always presented in random order. This second rating was explained as necessary for establishing a stability index since opinions on controversial issues are, of course, subject to daily fluctuations. Upon completion, the questionnaires were collected, and the subjects were then given a demanding multiple-choice test for comprehension. This test contained three questions for each of the communications. Since the messages had been randomly distributed, it was necessary to present questions for all eight issues to each subject; however, subjects were instructed to answer only those questions relating to issues on which they had read messages. At the end of the experiment the true purpose of the study was explained in detail and all questions answered.

The two forms of pretest and the two "tuning" conditions constituted a 2 × 2 factorial design. Since the authors were primarily interested in drawing inferences about the effects of the experimental manipulations on the reactions of subjects to persuasive communications, the subject was used as the unit of sampling in the statistical analyses. All statistical tests reported are two-tailed unless otherwise specified. In order to relate the Socratic effect to different levels of ability, the American College Test scores for each subject were obtained from his cumulative record file.

## Results and Discussion

### *Socratic Effects*

Limitations on the amount of time subjects were available required the omission of the desirability ratings which were included by McGuire (1960b, 1960c) in his measure of the Socratic effect as a correction for "wishful thinking." Consequently, an alternative procedure was used which had been employed by Dillehay et al. (1966), whereby the rank-order correlations were computed between the product of the probability ratings on the premises and the ratings on the conclusions for the no-message control syllogisms. In the first analysis, separate correlations were obtained for each of the four experimental conditions on both the initial and final test data irrespective of the subjects' ability levels.

Those subjects to whom the initial questionnaire was presented with the propositions in random order showed a slight, but totally insignificant, trend toward greater consistency on the second testing (.49 to .61 in the communicator condition and .50 to .60 in the receiver condition). Subjects who received the intact syllogisms on the pretest showed trivial negative changes (.63 to .56 for communicators and .58 to .57 for receivers). These correlations clearly offer no evidence of a Socratic effect.

To test for the Socratic effect at different ability levels, subjects were partitioned at the median into high- and low-ability groups on the basis of their American College Test scores. Since the tuning manipulation appeared irrelevant to the question of a Socratic effect, correlations were computed for the four conditions generated by two levels of ability

TABLE 1

Rank-Order Correlations between the Joint Probabilities of the Major and Minor Premises and Conclusions and the Mean Departures from Perfect Consistency for the No-Message Control Syllogisms

| Item | Random-order pretest | | Syllogistic-order pretest | |
|---|---|---|---|---|
| | High ability | Low ability | High ability | Low ability |
| Initial test data | | | | |
| $p(c) - p(a)\,p(b)$ | 17.44 | 25.98 | 16.29 | 19.15 |
| $\rho$ | .66 | .31 | .65 | .58 |
| Final test data | | | | |
| $p(c) - p(a)\,p(b)$ | 15.48 | 19.35 | 17.69 | 20.62 |
| $\rho$ | .72 | .57 | .62 | .55 |

Note.—In the departure from perfect consistency measure, $p(c) - p(a)\,p(b)$, low scores indicate greater consistency.

(above and below the median) and the two forms of the pretest. These data are presented in Table 1. In the random-order groups, the rank-order correlations for the high-ability subjects revealed a negligible increase toward consistency over the two presentations (.66 to .72). In contrast, for the low-ability subjects the posttest correlation of .57 was considerably, but not significantly, higher than that of the pretest rho of .31. However, even this marked increase failed to raise this group to the consistency level evinced by the high-ability subjects. Those persons who received the intact syllogisms on the pretest showed no tendency at either ability level toward greater consistency on the posttest.

Since the rank-order correlation is rather insensitive to small changes in the magnitude of discrepancy between the probability ratings given the conclusions and the joint probabilities of the premises, the Socratic effect was also examined in terms of the subjects' departure from perfect logical consistency on each of the two testings. To the extent that their probability ratings on the three components of the syllogisms more closely approach perfect consistency—that is, $p(c) = p(a) \times p(b)$—on the second testing than on the first, they have demonstrated a Socratic effect, assuming that the unknown $p(k)$ factor[3] remains relatively constant across the two testings. The initial and final mean departures from perfect consistency (based on absolute values) are shown in Table 1. It can be seen that the groups differed in initial levels of consistency, with low-ability subjects in the random-order pretest condition responding less consistently ($t = 2.06$, $p < .05$) than their counterparts in the high-salience condition. For the high-ability subjects the form of pretest made no appreciable difference.

On the second testing the low-ability random-order pretest group had changed significantly in the direction of greater consistency ($t = 2.19$, $p < .05$), while changes for the other three groups were trivial, all yielding $t$ values of less than 1. Interpreting this shift toward greater consistency in terms of a Socratic effect, however, is perilous since it is also in the direction expected from regression. Moreover, the somewhat selective choice of comparison cells in each of the foregoing analyses may have contributed to the borderline significance obtained. The higher level of initial consistency shown by low-ability subjects who received the intact-syllogism form of the pretest, compared to their counterparts who received the syllogistic components in random order, offers some degree of validation for the pretest manipulation of salience, although it is not clear why the effect was limited to low-ability subjects. It is possible that the high-ability subjects entered the experimental situation with a markedly different $p(k)$ factor due to more extensive information. Consequently, if a greater number of alternative sets of premises led to the conclusion of a syllogism with a given probability for the high-ability subjects, increasing the salience of the specific premises involved in the study would have less impact since they would represent a smaller proportion of their universe of related beliefs and the alternative sets would remain unchanged. It is also likely that the belief systems of higher ability subjects were initially less distorted by wishful thinking, thereby leaving less room to change in the direction of greater rationality as a function of increasing the salience of logical relationships. Unfortunately, in the absence of the desirability ratings of these propositions, this must remain conjecture.[4] These factors might also explain the absence of a Socratic effect at higher ability levels; although, in the present study, the

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[3] $p(k)$ is defined as the subjective probability of occurrence of all states of affairs, other than the conjunction of $a$ and $b$, that would imply $c$ (McGuire, 1960a, p. 105).

[4] William J. McGuire (personal communication) disapproves of this method of analyzing the Socratic effect without consideration of the desirability of the propositions because it compounds the problem of $p(k)$. That is, in the absence of the desirability ratings, it is impossible to determine to what extent excess probability of the conclusion over the joint probabilities of the premises represents "wishful thinking," and to what extent the fact that alternative sets of premises in the subjects' cognitive systems account for the higher probability of the conclusion. For a more detailed exposition of this point, see McGuire (1960a), especially the Appendix. The authors agree that McGuire's procedure is more appropriate and these ambiguities dramatically illustrate the point.

TABLE 2

INITIAL MEANS AND SUBSEQUENT CHANGES ON THE MINOR AND MAJOR PREMISES AND CONCLUSIONS FOR EACH OF THE EXPERIMENTAL CONDITIONS

| Item | Syllogistic-order communicators | | | Syllogistic-order receivers | | | Random-order communicators | | | Random-order receivers | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Major | Minor | Conclusions | Major | Minor | Conclusions | Major | Minor | Conclusions | Major | Minor | Conclusions |
| Pretest *M* | 61.53 | 63.59 | 44.61 | 60.26 | 61.80 | 44.87 | 61.03 | 63.08 | 45.38 | 59.49 | 60.77 | 41.03 |
| *M* changes | 3.08 | 17.69 | 17.18 | −2.82 | 10.51 | 6.41 | 12.82 | 29.23 | 20.00 | 4.62 | 17.44 | 9.74 |

Note.—There were 13 subjects in each of the four conditions.

evidence for a Socratic effect at *any* ability level was tenuous indeed.

### *Direct Impact of Messages*

The effects of the independent variables can be seen in Table 2, which contains the initial means and subsequent changes for each of the four experimental conditions. Computing these means across issues was justifiable since each of the issues appeared approximately the same number of times in each condition.[5]

Although subjects were randomly assigned to conditions, it will be recalled that the pretest contained one of the manipulations. Hence, prescores of subjects in the intact-syllogism form of the questionnaire might differ from those in the random condition. It can be seen in Table 2 that the four experimental conditions did not differ appreciably in initial mean probability ratings for the premises and conclusions (the *F* ratio for treatments was less than 1), nor did they differ in initial levels of consistency (to be discussed subsequently). Therefore, the differential effects of the independent variables were tested by an analysis of variance of the difference scores (i.e., the posttest scores minus the pretest scores). In addition, the mean change on the minor (target) premise in each of the four conditions was compared to the change of 2.76 points in the controls using Dunnett's (1955) technique for multiple comparisons with a control. The messages in all experimental conditions had a significant persuasive impact ($p < .05$).

As predicted, subjects in the high-salience condition (intact syllogisms on the pretest) changed significantly less on the minor premises ($F = 5.43$, $p < .05$) than those in the low-salience condition (random form of the pretest). This effect was expected since the increased salience of the logical relationship between the conclusions and the minor premises should increase the degree of cognitive imbalance experienced by the subject if he changes on one proposition without changing on the other. Indeed, McGuire (1960a) demonstrated that it is, at times, easier to convince subjects that an undesirable, rather than a desirable, event is likely to occur when this change increases the overall level of cognitive consistency. Surprisingly, however, Nelson (1968) found that the number of values (three versus nine) to which a belief was anchored made no difference in conferring resistance to persuasion, whereas it clearly should have varied the degree of imbalance.

A main effect was also obtained for the tuning method of induction, but its direction was opposite to that predicted, with communicators changing more than the receivers toward the position advocated by the messages ($F = 5.74$, $p < .05$). The interaction between these two variables was virtually nil ($F < 1.0$). Hence, this manipulation facilitated opinion change in the very condition where integration of beliefs should have been maximum. Since the prospect of having to communicate the material to others may have motivated subjects in this group to learn the material more thoroughly, differences in learning might explain their greater opinion change (e.g., Watts & McGuire, 1964). No support was obtained for this interpretation, however, as the means on the test of recall of message content were virtually identical for the four groups, varying from 1.43 to 1.56 with a possible range of 4 points ($F < 1.0$). Furthermore, in this study there was little relationship between learning and opinion change, with the correlations in the four conditions varying from $-.18$ to $+.14$. These negligible correlations between learning of content and opinion change might be due to the multiple-choice items being quite specific and difficult. Thus, subjects may have fully grasped the point of the message without learning all of the details and, consequently, tended to score low on the test. On the other hand, investigators have frequently pointed out (e.g., Hovland, Janis, & Kelley, 1953; McGuire, in press) that comprehension is no guarantee of opinion change, but normally a prerequisite; and Greenwald (1969) suggested that the relationship between learning of content and opinion change is mediated by the recipient-generated cognitions elicited by the messages.

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[5] The random assignment of issues to subjects resulted in extremely disproportional cell frequencies for two of the issues. Consequently, these two issues were dropped from the analysis, which left virtually identical representation of each issue in every experimental condition. In addition, the data for one subject in the random-order, receiver condition was randomly discarded in order to obtain an equal number of subjects in each cell.

A plausible interpretation of the tuning effect involves a distraction hypothesis (e.g., Festinger & Maccoby, 1964; Freedman & Sears, 1965). This interpretation assumes that concentrating upon the message content in order to communicate effectively to others distracts the subject from evaluating the content with respect to his own beliefs. This is somewhat analogous to Janis and Gilmore's (1965) interpretation of the effectiveness of active participation in producing opinion change in terms of "biased scanning." That is, the task motivates the subject to focus upon the points made in the communication rather than considering possible counterarguments.

Indeed, earlier investigators (e.g., Cohen, 1961) have found that subjects expecting to communicate tend to polarize impressions while ignoring contradictory information. On this basis, greater opinion change for communicators might have been predicted had it not been assumed that the greater degree of cognitive integration would tend to anchor the belief attacked to the logically related, but unmentioned, beliefs. In this reasoning, the fact was overlooked that the effectiveness of these related beliefs as anchors should depend upon such factors as their importance to the subject and their degree of centrality (e.g., Pilisuk, 1962). In general, it is unlikely that propositions of the syllogisms used in the present study represented cherished, or even important, beliefs to the subjects; hence, their anchoring effects were probably slight. Under these conditions, the main impediment to subjects' changing consistently on the belief attacked *and* the related beliefs, thus achieving consistency at a different level (which appears to have happened when the indirect impact is considered), is the involvement of more cognitive work in adjusting the premises in order to be consistent than in resisting change on the premise attacked. However, Rosenberg and Abelson (1960) have already shown that this "least effort" principle is rather easily subjugated to other sources of motivation. Therefore, the facilitative effects of the communicator induction on opinion change appears to have easily outweighed the cognitive work aspect leading to consistently greater change; while, as predicted, within each of the tuning conditions, the syllogistic order of pretest retarded change on the target premise. While these are admittedly speculations, they are amenable to empirical investigation through such manipulations as varying the importance of the anchoring beliefs.

### *Indirect Impact of the Persuasive Messages*

It was anticipated that change on the logically related, but unmentioned, conclusions would be enhanced by the communicator tuning as well as by the syllogistic order of presentation of the pretest. Since the prescores of the four groups were comparable ($F < 1.0$, see Table 2), an analysis of variance was performed on the difference scores. This analysis showed a significantly greater change on the conclusions among communicators than among receivers ($F = 10.25$, $p < .005$). Furthermore, subjects in each of the communicator conditions changed significantly on the unmentioned conclusions in the direction advocated for the minor premises compared to the mean change of 3.08 for the controls ($p < .001$) using Dunnett's (1955) method; whereas subjects in the receiver conditions did not differ significantly from the controls ($t = 1.63$ for the random-order pretest and $t = .81$ for the syllogistic-order pretest). The effect of the form of pretest did not approach significance ($F < 1.0$), nor did the interaction between these two variables ($F < 1.0$). This tuning effect closely parallels that obtained on the minor (target) premises in that subjects expecting to communicate show greater change on each measure than their receiver counterparts.

*Changes toward greater consistency on the message syllogisms.* The experimental hypothesis clearly implies that the final level of consistency attained on the syllogisms should be related to the salience of the logical relationships among the components. That is, when salience or integration is high, it was predicted that subjects would either resist change on the specific belief attacked or change concomitantly on the logically related beliefs. Therefore, the data were analyzed in terms of changes toward consistency for the message syllogisms as had been done for the control syllogisms in measuring the Socratic effect. The mean departure from perfect consistency (based on absolute differences) and the rank-order correlations between the product of the ratings given to the premises and the conclusions are presented in Table 3.

First, considering the departure from perfect consistency—that is, $p(c) - p(a) \times p(b)$—it can be seen in Table 3 that the four groups did not initially differ in this respect ($F < 1.0$). However, following receipt of the persuasive messages, the communicator groups showed a marked change toward greater consistency; whereas the changes for the receiver groups were of much smaller magnitude with the direction being away from consistency for one group. An analysis of variance of the change scores showed the tuning effect to be significant ($F = 4.86$, $p < .05$), while neither the main effect of pretest nor the interaction approached significance ($F\text{s} < 1$). The rank-order correlations presented in Table 3 show a similar pattern, although the differences between correlations did not reach significance.

TABLE 3

RANK-ORDER CORRELATIONS BETWEEN THE JOINT PROBABILITIES OF THE MAJOR AND MINOR PREMISES AND CONCLUSIONS AND THE MEAN DEPARTURES FROM PERFECT CONSISTENCY FOR THE MESSAGE SYLLOGISMS

| Item | Random-order pretest | | Syllogistic-order pretest | |
|---|---|---|---|---|
| | Communicators | Receivers | Communicators | Receivers |
| Initial test data | | | | |
| $p(c) - p(a)\,p(b)$ | 20.05 | 20.38 | 20.49 | 19.23 |
| $\rho$ | .53 | .51 | .50 | .52 |
| Final test data | | | | |
| $p(c) - p(a)\,p(b)$ | 13.92 | 21.59 | 13.44 | 16.70 |
| $\rho$ | .72 | .46 | .76 | .63 |

It is easy to theorize why the tuning manipulation should be more powerful than the order of presentation of the syllogistic components on the pretest inasmuch as it directly preceded opinion measurement, whereas the pretest manipulation had been implemented 2 days previously. Hence, much of the pretest effect may have dissipated during the interim. However, this cannot explain why the pretest manipulation significantly inhibited change on the target premises, but had no effect on the logical repercussions of the persuasive messages. Possibly, the resistance to change that it conferred on the minor premises was artifactual rather than the result of anchoring effects created by the increased salience. That is, the presentation of the premises as intact syllogisms on the pretest may have altered the "demand characteristics" (e.g., Orne, 1962; Rosenberg, 1965) of the experiment, either heightening the subjects' awareness that the study involved persuasion or causing them to discount the messages because only one aspect of a "complex" issue was touched upon (i.e., only one premise of the syllogism). Alternatively, the intact-syllogism form of the pretest may have increased the subjects' motivation to appear consistent on both administrations of the test, thus producing the obtained result. While this latter interpretation is intuitively appealing, in the authors' opinion, it is not compelling since, for this group (intact-syllogism pretest), the format of the final opinionnaire was markedly different from that of the first session, which should make it more difficult to respond consistently in both cases.

In combination, these analyses indicate

that compared to receivers, the subjects expecting to communicate information to others change more on the specific belief discussed in the persuasive message and on logically related, but unmentioned, beliefs. Furthermore, these changes are very consistent in that after the initial imbalance introduced by the new information, subjects in the communicator conditions realign their beliefs in order to be more consistent (in terms of the model) than when initially tested ($p < .05$) and are significantly more consistent than their receiver counterparts. These findings are quite congruent with Cohen's (1961) theory and findings that communicators tend to form compact, integrated "packages" of information to facilitate transmission to others.

*Actual versus logically required change on the conclusions.* It is also of interest to examine the amount of change obtained on the conclusions compared to the amount of change logically required on the basis of observed changes on the major and minor premises; for example,

$$\Delta p(c) = [1 - p(k)]\,[\Delta p(a)p(b) + \Delta p(b)p(a) + \Delta p(a)\Delta p(b)].^6$$

Since $p(k)$ is unknown, the predicted amount of change remains an approximation being equal to, or less than, the term in brackets. McGuire (1960b) theorized that cognitive systems are characterized by inertia and, on this basis, predicted and found that the obtained change in the conclusions was significantly less than that logically required by the above formula. Dillehay et al. (1966) replicated this finding in one experiment but failed to do so in the other. In the present study, only one of the four conditions (random-order, communicator) yielded significantly less change ($t = 2.75$, $p < .01$) on the conclusions than logically required by the model. This would appear to be due to the extreme amount of change necessary (29.67 points, computed from the data in Table 2, letting $p(k) = 0$) because of the large change on the major premises in this condition (to be discussed subsequently).

### *Intellectual Ability and Opinion Change*

Higher levels of intellectual ability should facilitate the indirect impact of a persuasive message since higher ability subjects would be more likely to see the connection between the arguments advanced and the unmentioned conclusion. Although Cooper and Dinerman (1951) found some evidence that intelligence was positively related to influence by an implicit message, Hovland and Mandell (1952), in a more carefully controlled experiment, found no relationship between intelligence and opinion change when the conclusion of the message was left implicit.

To examine this relationship, correlations were computed between the subjects' American College Test ability scores and changes on the minor (target) premises and the unmentioned conclusions. For the conclusions, these correlations were small and insignificant, ranging from .21 in the random-order, receiver group to −.12 in the syllogistic-order, communicator group. They offer little evidence that the logical repercussions of persuasive messages are dependent upon ability level. Analyses of the direct effects of the persuasive messages (change on the minor premises) yield a more interesting pattern of results. For each of the communicator conditions, moderately strong positive correlations were obtained between ability and opinion change ($r = .51$, $p < .05$ and $r = .28$, $p < .10$ for the random-order and syllogistic-order pretest groups, respectively); whereas, for the receiver conditions, these values had decreased to $r = .06$ in the random-order condition and $r = -.01$ in the intact-syllogism form of pretest. Averaging the two correlations obtained in the communicator conditions and those in the receiver conditions by means of a $z$ transformation resulted in an average $r$ of .40 for communicators and one of .025 for receivers. These values differ significantly from one another ($z = 2.40$, $p < .05$).[7] The interpretation for this rather unexpected finding is as follows: Persons expecting to communi-

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[6] The authors thank William J. McGuire for calling attention to the fact that the formula for logically required change, as originally stated, was incorrect because it did not include $1 - p(k)$ term. Hence, it tended to overestimate the amount of required change.

[7] For the purpose of averaging these correlations, product-moment $r$'s were computed instead of the rhos used elsewhere.

cate to others would be more inclined to think of additional arguments supporting the position than would receivers who were to wait passively for more information. This ability to generate new arguments should logically vary with intelligence. Furthermore, subjects of low intellectual ability might well feel threatened by the task of communicating to others, thus experiencing rather negative recipient-generated cognitions (Greenwald, 1969) as well as being unable to use effectively the information provided by the messages.

### *Illogical Changes on the Major Premises*

An unexpected finding was the large change on the major premises in the random-order, communicator condition. Although McGuire (1960b) found that the greatest change toward consistency in the Socratic effect occurred on the major premises, interpretation of the change observed in the present study (see Table 2) in terms of a Socratic reorganization prior to the receipt of the messages is not compelling since it could hardly explain the fact that the change in this condition so greatly exceeds that obtained in the other experimental and control conditions. Rather, some aspect of this experimental condition appears to have induced sizable change in all components of the syllogisms.

Using linear syllogisms, De Soto, London, and Handel (1965) found the number of illogical responses (or errors) to be a function of the order of presentation of the elements of the syllogism. Therefore, the data were partitioned according to the order of appearance of the three propositions of each syllogism on the final test even though McGuire (1960b) had found that order of elicitation of the components in the first session made no difference in the Socratic effect. This procedure automatically partitioned subjects in the random-order pretest conditions on the same basis for the initial test as well, since the format of their pre- and postquestionnaires were identical; whereas, for subjects receiving the syllogistic-order pretest, the sequence on the initial test was, of course, fixed. Order of presentation of the syllogistic components on the final test had no effect on subjects receiving the intact-syllogism form of the pretest, whereas it had a striking effect on those subjects in the random-order pretest conditions (see Table 4).

TABLE 4

MEAN CHANGE ON THE MAJOR PREMISE AS A FUNCTION OF ITS ORDER OF APPEARANCE IN THE SYLLOGISM

| Item | Order in which the major premise appeared on the questionnaire | | | |
|---|---|---|---|---|
| | 1st | Middle preceded by minor premise | Middle preceded by conclusion | Last |
| Pretest *M* | 65.71 | 67.69 | 58.46 | 48.80 |
| *M* changes | 2.14 | 11.54 | 4.62 | 17.08 |

Note.—Random-order pretest conditions only.

Analysis of change on the major premises is complicated, however, by substantial differences in initial probabilities assigned to the major premises in the different order-of-appearance conditions. A 2 × 2 analysis of variance of the initial ratings (with the data partitioned according to whether the major premise was preceded by the minor premise or the conclusion independently) showed a significant main effect for position of the conclusions ($F = 4.27$, $p < .05$); while the main effects of the minor premise and the interaction were trivial ($F < 1$ and 1.50, respectively).[8] In this study, the order of appearance of the major premise is confounded with the syllogisms since the elements were not systematically manipulated. A comparison of these initial ratings with the corresponding premises in the syllogistic-order pretest condition (where the major premise always came first) reveals that the differences are apparently due to the particular subset of syllogisms involved, rather than the order of appearance of the components. However, a similar comparison of the differential change observed in the major premises according to the order of elicitation indicates that it cannot plausibly be attributed to differences among syllogisms. An analysis of variance of the change scores on the major premise yields a significant main effect ($F = 5.49$, $p < .05$) due to position of the minor premise, with the magnitude of change being greater when it precedes, rather than follows, the major premise; while the $F$ ratio for position of conclusions was only 1.92 and that for the interaction, less than 1. Even though the initial ratings differed significantly, regression can hardly be the explanation for this effect since the initial differences occurred as a function of the order of appearance of the conclusions and the differential change resulted from the order of elicitation of the minor premises and was unrelated to the conclusions. With respect to position of the minor premise, the marginals for initial ratings were quite similar, regardless of whether the minor premise preceded or followed the major premise ($F < 1.0$). Furthermore, when the major premise appeared in the middle, preceded by the minor premise, the change of 11.54 points differed significantly ($t = 2.21$, $p < .05$) from the 2.14-point change when the major premise appeared first, even though the initial means were virtually identical. This relationship would appear to be linked to the pretest (or some type of pretest-posttest interaction) since it did not appear among subjects receiving the intact-syllogism form of the pretest even though their posttests were the same as their counterparts in the random-order pretest condition. The difference could either be due to the fact that the major premise always came first in the syllogistic-order pretest (an arrangement which resulted in minimal change on it; see Table 4) or to the increased salience of logical relationships for these subjects.

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[8] Since partitioning the data on this basis resulted in disproportional cell frequencies, the significance levels were estimated by the technique described by Walker and Lev (1953, pp. 381–382).

While this finding is not without interest, it must be considered for the present as merely suggestive since it was not systematically examined. It does, however, suggest a discrepancy from the logical model since, regardless of whether the major premise preceded or followed the minor premise, the probability of its truth was independent of the minor premise; yet, when it occurred later in the sequence, change on the minor premise generalized to it.

McGuire (1960a) has discussed a number of such logical fallacies in human thinking and Dillehay et al. (1966) have suggested that the replicable change on unmentioned, but related, issues may be due to the "extralogical" cognitive relatedness of the propositions, rather than their logical structure. However, Peterson and Beach (1967, pp. 36–37) pointed out that experiments testing the consistency of probability estimates for the joint occurrence of two independent events, or even the more complicated conditional probability case where the events are dependent, provide results quite consistent with probability theory. In situations more analogous to the present study, where revisions of probability estimates are required after change has been introduced into the system, the results again closely approximate those expected from probability theory in all but the most complicated situations.

Regardless of whether this model, or a more "psychological" one, ultimately proves to be more isomorphic with human behavior, it is clearly of considerable heuristic value.

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# AFFECTUAL DYNAMICS IN SIMPLE SENTENCES[1]

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Original attitudes toward words predict attitudes resulting when the words are combined into simple assertions. For example, attitudes toward "father," "kill," and "uncle" predict how one feels toward all three after the assertion "the father killed the uncle." The prediction equation involves terms for the original subject attitude ($S$), the original verb attitude ($V$), and the interaction of $V$ and object attitudes ($Q$). The level of prediction is high, with original attitudes accounting for two-thirds of the total variance in resultant attitudes. Activity connotations can be predicted in the same way, except that a $V \times Q$ interaction term is unnecessary. Potency connotations also appear to be predictable, but the obtained data do not permit firm generalizations.

Recent research by Heider (1967) and Gollob (1965, 1968) has set attitude-balance theory in a linguistic framework, with attitude change being analyzed as a function of attitude combination in sentences. A basic innovation in the approach is recognizing that verbs as well as nouns have attitudinal associations, and so a sentence like "The Senator kissed the baby" is to be analyzed in terms of *three* attitudes rather than in terms of two attitude objects which are associated or dissociated. The verb attitude enters into attitude dynamics both additively and through a multiplicative interaction with the object. Gollob's research suggests that considering verb attitudes explicitly and quantitatively yields attitude-change predictions of high accuracy.

The present study builds on the insights developed by Heider and Gollob, replicating some of their research and extending it in several directions. Since Gollob's study is the more quantitative, it serves as the more important guideline and comparison standard for analyses here. An effort is made to replicate Gollob's finding that the attitude toward the subject of a sentence can be predicted from the attitudes associated with each word of the sentence. However, in the present study the original word attitudes are measured directly rather than inferred as in Gollob's analyses. In addition, an attempt is made to show that changes in the attitudes toward sentence verbs and objects also can be predicted in terms of the original attitudes that enter into a sentence. Indeed, it is proposed that a single formula is adequate for predicting attitude shifts for subject, verb, and object. Finally, the present study deals not only with evaluation reactions, but also with two other dimensions of affective response—activity and potency (Osgood, 1962).

## METHOD

### *Prediction Models*

The procedure in the present study is to postulate several models, or sets of equations, that might be used to predict attitudinal dynamics in sentences. The models range from simple to complex, and the major goal of analyses is to evaluate the different models in terms of their predictive adequacy and their parsimony. The preferred models then are examined further to determine their implications concerning attitudinal dynamics.

Each of the equations given subsequently is a possible basis for predicting the attitudes that result from simple assertions. An equation specifies how measurements of the original attitudes for the words in the sentence are to be processed to estimate the new, or resultant, attitudes that occur as a consequence of the assertion. An "original attitude" is defined here as the average attitudinal rating of a word when the word is placed in a neutral context. A neutral context is a sentence consisting of function words except for the stimulus word, for example: "She is a *wife*; He *kicked* it." A "resultant attitude" is defined as the average rating of a word in its non-neutral context, such as: "The *wife* kicked her husband."

The simple sentences considered here consist of a subject, a transitive verb, and an object. The original attitude associated with the word in the

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subject slot is represented subsequently as $S$, and the resultant attitude is $S'$. Similarly, the original and resultant attitudes for the words in the verb and object positions are $V$ and $V'$ and $Q$ and $Q'$, respectively. ($Q$ rather than $O$ is used to avoid later confusion with zero.)

*Model I.* The simplest model for predicting resultant attitudes from original attitudes derives from assuming that attitudes may shift in different sentences, but such shifts are unpredictable from knowing the other attitudes evoked in the same sentence. In such a case, the best quantitative predictor of a resultant attitude for a word is merely the original attitude for that word, which gives the following model:

$$\begin{aligned} S' &= K_s + a_s S \\ V' &= K_v + b_v V \\ Q' &= K_q + c_q Q \end{aligned} \qquad [\text{I}]$$

The first formula indicates that the mean rating of the subject word in context ($S'$) is estimated to be some constant ($K_s$) plus the mean rating of the same word in a neutral context ($S$) weighted by a constant factor ($a_s$). Similarly, the verb resultant ($V'$) is some constant ($K_v$) added to the weighted ($b_v$) value of the original attitude toward the verb ($V$). The formulas are recognizable as regression-type equations, and, indeed, models are subsequently related to data through regression procedures. Error terms, indicating that the equations predict imperfectly, have been dropped here for simplicity. Model I is of little interest theoretically, but it provides a lower base line for evaluating other models.

*Model II.* Another possibility is that all of the attitudes evoked in a sentence do operate together to produce a resultant, but the resultant is a simple additive function of the original attitudes. This contrasts with Model I by suggesting that to predict, say, $S'$, one should consider not only $S$ but also $V$ and $Q$. It contrasts with later models by postulating the absence of significant interactions. Equations representing the model are as follows:

$$\begin{aligned} S' &= K_s + a_s S + b_s V + c_s Q \\ V' &= K_v + a_v S + b_v V + c_v Q \\ Q' &= K_q + a_q S + b_q V + c_q Q \end{aligned} \qquad [\text{II}]$$

*Model III.* Gollob's work (1968) suggests that Model II can be improved by adding a $V \times Q$ interaction term to the equations in Model II. This also reflects somewhat the stance of traditional balance theories that postulate that the overall configuration of attitudes can be crucial in determining attitude dynamics. The formulas below allow for a $V \times Q$ interaction as a linear function of the product of $V$ and $Q$. This means that when the verb and object differ in attitudinal direction (one good and the other bad), a decrement occurs in the resultants, whereas if they are both in the same direction (both good or both bad), an increment is added.

$$\begin{aligned} S' &= K_s + a_s S + b_s V + c_s Q + d_s(V \times Q) \\ V' &= K_v + a_v S + b_v V + c_t Q + d_v(V \times Q) \\ Q' &= K_Q + a_Q S + b_Q V + c_Q Q + d_Q(V \times Q) \end{aligned} \qquad [\text{III}]$$

*Model IV.* Gollob's analyses suggest that the $V \times Q$ interaction is the only interaction term that contributes much to attitudinal resultants. However, it is desirable to see if the finding can be replicated. Accordingly, a Model IV is created in which terms are included for every possible interaction. Since Model IV encompasses all previous models and is more exhaustive than any of the others, it provides a standard for prediction by which the other models can be measured.

$$\begin{aligned} S' = {} & K_s + a_s S + b_s V + c_s Q + d_s(V \times Q) \\ & + e_s(S \times V) + f_s(S \times Q) + g_s(S \times V \times Q) \\ V' = {} & K_v + a_v S + b_v V + c_v Q + d_v(V \times Q) \\ & + e_v(S \times V) + f_v(S \times Q) + g_v(S \times V \times Q) \\ Q' = {} & K_q + a_q S + b_q V + c_q Q + d_q(V \times Q) \\ & + e_q(S \times V) + f_q(S \times Q) + g_q(S \times V \times Q) \end{aligned} \qquad [\text{IV}]$$

## *Procedure*

*Stimulus words and sentences.* With a positive or negative word in the subject, verb, and object positions, there are eight types of attitudinal configurations that a simple sentence can present (+++, ++−, +−+, etc.). The basic study design was to create three sentences to represent each pattern. In this way, the variation in types of sentences was maximized and there was maximum opportunity for discovering interactions.

A working vocabulary of nouns and verbs was selected from an available dictionary of semantic differential ratings (Heise, 1965). To be included in the vocabulary a word had to have (*a*) a reported evaluation score above +1 or below −1; (*b*) relatively low absolute scores on the activity and potency dimensions. The selection procedure insured large variation in the evaluation ratings of the words while minimizing possible contamination effects from the other two dimensions of affective response. The vocabulary words were used then to create a set of plausible sentences in accordance with the study design. That is, three sentences were written to represent each of the eight basic attitudinal configurations (+++, ++−, +−+, etc.).[3] An effort was made to obtain a wide variety of sentence types; for example, some of the sentences were interpersonal in nature ("The man joined his wife"), but others were not ("The prices caused trouble"). Twenty-seven different words were used to construct the sentences: 14 nouns and 13 verbs. Henceforth, these are called the stimulus words. The complete set of stimulus sentences is presented in Table 2 in the Results section.

*Attitudinal ratings.* Attitudinal ratings were obtained using a semantic differential consisting of three scales: *nice–awful, sweet–sour,* and *good–bad.* The scales are known to be fairly general in their applicability and they are reasonably pure measures of evaluation (Jakobovits, 1966). A standard format for the scales was used with seven rating positions on each scale, such positions being labeled

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[3] One evaluation sentence, a −+−, was dropped accidentally in typing the questionnaire.

"extremely," "quite," "slightly," or "neutral." In each presentation of the scales, two scales were oriented with their "good" poles on the right side of the page, and the third scale was oriented with its "good" pole on the left.

Ratings of original attitudes were obtained by presenting each word in a neutral sentence, that is, a sentence consisting otherwise of function words only. The ratings of attitudes in a neutral context were obtained from the same subjects and at the same time as other ratings. The ratings of words in non-neutral contexts were obtained by presenting a stimulus sentence, underlining one of its component words, and instructing subjects as follows (previous instruction having already been given on rating with the semantic differential):

> Think of the sentence as describing an event—an event unrelated to any of the others described on the same page. Now, taking this event into account, how do you feel toward the person, thing, or act that is underlined. Rate the underlined word on the three adjective scales. Remember, we want to know your feelings, taking this particular event into account.

Each page of a stimulus booklet had seven stimulus sentences on it and seven sets of rating scales. About one-third of each test booklet was devoted to evaluation ratings, the other two-thirds being devoted to activity and potency stimuli. Pages of the booklets were shuffled so that subjects alternated among evaluation, activity, and potency ratings.

TABLE 1

ALLOCATION OF STIMULUS WORDS TO THE THREE SAMPLES OF SUBJECTS: A, B, AND C

| Rated in context of: | Subject (nouns) | Verb | Object (nouns) |
|---|---|---|---|
| Evaluation stimuli | | | |
| Neutral sentence | B | C | B |
| Stimulus sentence | A | B | C |
| Activity stimuli | | | |
| Neutral sentence | A | B | A |
| Stimulus sentence | C | A | B |
| Potency stimuli | | | |
| Neutral sentence | C | A | C |
| Stimulus sentence | B | C | A |

Note.—A: $n = 37$; B: $n = 36$; C: $n = 33$.

Three different booklets were made up and distributed to three different subject samples so that no subject would rate the same word twice (i.e., no subject rated a word both in a neutral and non-neutral context) and so that no subject would rate more than one word in a single stimulus sentence. The overall allocation of subjects to evaluation stimuli is indicated in Table 1. The design was identical for each evaluation sentence.

*Subjects.* Subjects were summer-session students enrolled either in an introductory sociology course or in an intermediate educational psychology course at a large Midwestern campus. Subjects from all age levels and in both courses were pooled for the analyses. Males and females also were pooled together since previous analyses by Heise (1966) had revealed only small sex differences in mean attitudinal responses to words. The three subsamples (A, B, C, in Table 1) were chosen randomly from the subject pool.

*Coding and preliminary calculations.* Ratings on each scale were coded $-3$ for extremely negative (bad) to $+3$ for extremely positive (good), with 0 as the assumed neutral point. The mean rating of a single stimulus by a single rater was obtained by averaging the ratings on the three separate semantic differential scales. The individual mean ratings, in turn, were converted to group mean ratings by averaging over all subjects who rated the particular stimulus.

Words were rated in neutral contexts by different subjects than those who rated them in the contexts of stimulus sentences, and so a simple $t$ test was employed to determine whether context created significant shifts in ratings. Out of 69 comparisons, 41 were significant at the .05 level in a two-tailed test. The tests are not all independent, but this is ample evidence that the stimulus sentences provoked attitude shifts and that the differences are reflected in the overall mean ratings. Henceforth, the unit of analysis is the overall mean rating of a word in a particular sentence context.

*Activity and potency.* Studies to explore the dynamics of activity and potency associations were carried out simultaneously with the study of evaluations described previously. Procedures for the activity and potency studies were completely parallel to those described previously. Additional sentences were created following the same general design, and the activity and potency stimulus-sentences were included in the same test booklets as the evaluation sentences, with the same subjects providing the ratings. (See Table 1 for the allocation of subjects to activity and potency ratings.)

In the activity study, 17 nouns and 9 verbs were used to create 24 stimulus sentences. The semantic differential scales used to obtain ratings were fast–slow, lively–still, and noisy–quiet. In coding, positive values were assigned to the active ends of the scales.

In the potency study, 12 nouns and 8 verbs were used to develop the stimulus sentences. The scales used to obtain potency ratings were powerful–power-

TABLE 2

Mean Evaluation Ratings of Words in Context of Neutral Sentences and Stimulus Sentences

| Stimulus sentences for evaluation study | Subject ratings |  | Verb ratings |  | Object ratings |  |
|---|---|---|---|---|---|---|
|  | Neutral | Stimulus | Neutral | Stimulus | Neutral | Stimulus |
| The uncle helped the father. | .99 | 1.80 | 2.30 | 1.77 | 1.67 | 1.76 |
| The enemy surrounded the crowd. | −1.95 | −1.86 | − .49 | −1.48 | − .05 | −1.43 |
| A church protected the thing. | 1.04 | 1.64 | 1.63 | 1.28 | − .15 | .67 |
| The problem troubled the uncle. | −1.12 | −1.33 | −1.52 | −1.28 | .99 | − .48 |
| The farms used up the land. | .98 | − .14 | − .13 | −1.02 | 1.41 | − .09 |
| The debts united the enemy. | −1.83 | − .51 | 1.69 | − .51 | −1.95 | −1.31 |
| The uncle caused the trouble. | .99 | −1.45 | − .44 | −1.52 | −1.67 | −1.95 |
| The crowd helped the father. | − .05 | 1.78 | 2.30 | 1.89 | 1.67 | 1.94 |
| The nation protected the churches. | .48 | 1.76 | 1.63 | 1.51 | 1.04 | 1.53 |
| The prices caused trouble. | − .05 | −1.55 | − .44 | −1.32 | −1.67 | −1.66 |
| The gardens saved the enemy. | 1.35 | − .06 | 2.18 | − .04 | −1.95 | −1.23 |
| The enemy burned the church. | −1.95 | −2.40 | −1.62 | −2.24 | 1.04 | −1.56 |
| The father shot the uncle. | 1.67 | −2.22 | −2.11 | −2.44 | .99 | −1.76 |
| The trouble helped the enemy. | −1.67 | −1.04 | 2.30 | −1.15 | −1.95 | −2.11 |
| The nation destroyed the enemy. | .48 | .86 | −1.95 | − .09 | −1.95 | .41 |
| The prices helped the farms. | − .05 | 1.60 | 2.30 | 1.78 | .98 | 1.81 |
| The farms saved the nation. | .98 | 2.15 | 2.18 | 2.04 | .48 | 2.32 |
| The price troubled the enemy. | − .05 | .80 | −1.52 | .64 | −1.95 | .77 |
| The debts troubled the father. | −1.83 | −1.75 | −1.52 | −1.71 | 1.67 | −1.01 |
| The father fixed the trouble. | 1.67 | 2.10 | 1.31 | 1.68 | −1.67 | 1.05 |
| The problem united the nation. | −1.12 | 1.08 | 1.69 | 1.28 | .48 | 1.85 |
| The church surrounded the garden. | 1.04 | 1.43 | − .48 | 1.04 | 1.35 | 1.48 |
| Gardens hid the enemy. | 1.35 | − .69 | − .39 | − .64 | −1.95 | −1.74 |

less, big–little, and strong–weak. The potent end of the scales was coded as positive.

*Adequacy of the samples of sentences.* Sentences are the units of analysis in the present study, so each model equation is fitted to data on the basis of 23 observations (24 for activity and potency). The small sample size suggests that the formulas might not be very accurate, but the factor is partly balanced by other considerations. First, since measurements were obtained by averaging across both scales and subjects, the standard errors of measurements are small, and relatively little variance due to measurement error confounds estimations. Second, the sentences were developed within the framework of a factorial design calling for the juxtaposition of fairly intense attitudes in the subject, verb, and object positions and also demanding every combination of positive and negative attitudes. The sentences, therefore, constitute a stratified sample from the domain of all possible attitudinal configurations, so meaningful variance is maximized, and independent effects can be separated with some confidence. Third, the words in each set of sentences, while highly variable on one dimension of affective response, are fairly homogeneous on the other two dimensions; thus, possible contamination of the results by irrelevant dynamics is minimized.

## Results

### Evaluation

Table 2 presents the 23 stimulus sentences used in the evaluation study, along with the mean evaluation rating for each word in a neutral context and the mean rating of the word in the context of the stimulus sentence. The quantities in Table 2 are the basic data for subsequent analyses.

Each model equation was fitted to the data in Table 2 by means of regression procedures. For example, the empirical form for the first equation in Model I was obtained by regressing $S'$ on the measurements of $S$. The regression equation provided the empirical, least-squares estimates of parameters in the theoretical equation. Similarly, the first equation of Model II was specified by a multiple regression of $S'$ on the $S$s, $V$s, and $Q$s in each sentence. Again, the regression equation provided numerical estimates of the constant and of the weighting factors in the theoretical equation.

The actual regression equations will be presented only for the most successful model. Comparisons and evaluations of the models are conducted in terms of coefficients of determination ($R^2$) which indicate concisely how well a particular equation predicts resultants. $R^2$ rather than $R$ (the coefficient of multiple correlation) is used, because $R^2$

TABLE 3

SUMMARY STATISTICS ($R^2$) FROM MULTIPLE REGRESSIONS BASED ON VARIOUS MODELS FOR PREDICTING EVALUATION RESULTANTS

| Dependent variable | Model I | Model II | Model III | Model IV |
|---|---|---|---|---|
| $S'$ | .20 | .56 | .70 | .71 |
| $V'$ | .48 | .59 | .75 | .77 |
| $Q'$ | .19 | .47 | .67 | .70 |

Note.—All regression coefficients are statistically significant, $p < .05$.

is conveniently interpretable as "the proportion of variance accounted for."

The summary statistics ($R^2$) from the regressions based on Model I are given in the first column of Table 3. It can be seen that Model I predicts attitudes, but not very effectively. Model I would be of interest only if the other models are not better, so attention shifts immediately to Model II.

The regression results for Models II are given in the second column of Table 3. It is evident that Model II does improve predictions substantially over Model I. Model II accounts for more than twice as much of the variance in $S'$ and $Q'$ as Model I does; the coefficient of determination for $V'$ also is increased substantially. Even though Model II contains more variables (i.e., is less parsimonious), the amount of added effectiveness justifies the greater complexity, and Model I is now discarded. Whether Model II is to be preferred over other models depends on the remaining results.

Model III adds a $V \times Q$ interaction term to each of the equations in Model II, so its equations each have one additional term. The regression results are shown in Column 3 of Table 3. Comparing the results for Models II and III, one again sees a sizable jump in prediction capability. Model III as compared to Model II accounts for an additional 14% of the $S$ variance, an additional 16% of the $V$ variance, and an additional 20% of the $Q$ variance. Thus, the one additional term consistently is justified, as Gollob (1968) indicated, and it seems reasonable to discard Model II in favor of Model III. The absolute levels of prediction obtained with Model III are quite high. However, again, final evaluation of the model depends on remaining results.

The summary statistics of Model IV indicate that including all interactions does not improve predictions significantly beyond those obtained with Model III. Having added in three additional interaction terms, one increases the explained variance in $S$ by 1%, in $V$ by 2%, and in $Q$ by 3%. These are meager differences. Indeed, increases of such order would be expected, because including more terms in the regression analysis causes more chance fluctuations in the sample to be "explained." Thus, Model IV does not serve as a preferred alternative to Model III.[4]

*A generalized model.* The Model III formulas for predicting resultant attitudes from original attitudes are given below with numerical estimates for the regression constants and $\beta$-weights. It should be noted that the formulas are set up in a form for predicting actual resultant attitudes, not changes in attitude. Also it should be noted that the equations are for converting *un*standardized semantic differential ratings of original attitudes into *un*standardized estimates of resultant ratings.

$$S' = -.15 + .37S + .55V + .07Q + .25VQ \quad [1]$$

$$V' = -.24 + .23S + .60V + .07Q + .25VQ \quad [2]$$

$$Q' = -.13 + .17S + .40V + .36Q + .30VQ \quad [3]$$

The most striking feature of Formulas 1–3 is the degree of parallelism across formulas. Each formula has a small negative constant, and from there on there is a simple weighted sum of the four basic terms. Even the $\beta$-weights are similar across equations if one allows that the original attitude for a word has an effect that is independent of sentence dynamics and so increases the value of one of the coefficients in each formula. For example, one might arbitrarily subtract .20 from the $\beta$-weight in each formula that is associated with the original attitude for the word being predicted. Then, the $\beta$-weights that result are quite similar for Formulas 1, 2, and 3.

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[4] In their presentation of congruity theory, Osgood and Tannenbaum (1955) argued that an attitude's impact on other attitudes is weighted by its absolute intensity. To test this notion, a modified version of Model III was developed in which each original attitude measurement was entered into calculations weighted by its polarization (absolute value). The proportions of variance explained ($R^2$) using the revised model were: .65 for $S'$; .71 for $V'$; and .64 for $Q'$. Clearly, no predictive power is gained in the revised model, although considerable parsimony is lost.

The parallelism suggests that there is a single dynamic principle operating here rather than separate dynamics for the subject, the verb, and the object. It appears that attitudes evoked by a sentence all tend to cluster around a single resultant point. From Equations 1–3, one can infer that the formula for this general resultant has the form of Equation 4.

$$R = K + aS + bV + c(V \times Q) \qquad [4]$$

It was observed previously that the original attitude for a word seems to affect the resultant attitude for the same word in excess of sentence dynamics. Then the expected resultant attitude for any *particular word* in a sentence should be calculable as:

$$W' = R + dW \qquad [5]$$

where $W$ is the original attitude for the word of interest and $W'$ is the resultant attitude for that word. (If one is predicting the $S'$ resultant, then $W = S$; for the $V'$, $W = V$; and to predict the $Q'$, $W = Q$.)

The parameters in Formulas 4 and 5 were estimated by combining the equations to get:

$$W' = K + aS + bV + c(V \times Q) + dW \qquad [6]$$

and calculating the multiple regression of all word resultants on the terms in the formula. The data used were the same as before except that now all resultant attitudes were regressed using the *same* formula so there were $3 \times 23 = 69$ observations. The procedure of pooling the data for $S'$, $V'$, and $Q'$ allows separating out the independent effect of $W$, because in two-thirds of the cases the predictors $W$ and $S$ are different, $W$ and $V$ are different, and $W$ and $Q$ are different.

The parameter estimates obtained from the regression procedure allow Equations 4 and 5 to be specified as follows.

$$R = -.19 + .16S + .44V + .28(V \times Q) \qquad [7]$$

$$W' = R + .24W \qquad [8]$$

Formula 8 predicts 67% of the variance in all the resultant attitudes, whereas the mean proportion of variance accounted for by using Model III separately for each set of components is .71.

### *Activity*

Summary statistics for predicting activity resultants using different models are presented in Table 4.

Model I predicts $S'$ rather well, but does only a mediocre job of $V'$ and $Q'$. Model II provides a distinct advance in prediction capability over Model I. An additional 14% of $S$ variance is explained; an additional 41% of $V$ variance; and an additional 30% of $Q$ variance. Thus, one can conclude that activity connotations of words do combine in sentences at least in the additive manner indicated by Model II.

Comparing the regression results for Models II and III, one is struck by the almost complete absence of improvement in predictions using Model III. Whereas the $V \times Q$ interaction was a very important term in predicting evaluation resultants, the term appears to be of negligible importance for activity. This also is verified by the actual regression equations: the weighting constant for the interaction term is about zero in all three formulas.

Regressions based on Model IV include all possible interactions. The summary results presented in Table 3 indicate that the added complexity yields only meager improvements in predictions. The additional variance explained is 3% for $S'$, 2% for $V'$, and 7% for $Q'$.

The conclusion is that the activity connotations of words do combine in sentences,

TABLE 4

SUMMARY STATISTICS ($R^2$) FROM MULTIPLE REGRESSIONS BASED ON VARIOUS MODELS FOR PREDICTING ACTIVITY RESULTANTS

| Dependent variables | Model I | Model II | Model III | Model IV |
|---|---|---|---|---|
| $S'$ | .61 | .75 | .75 | .78 |
| $V'$ | .23 | .64 | .64 | .66 |
| $Q'$ | .37 | .67 | .68 | .74 |

Note.—All regression coefficients are statistically significant, $p < .05$.

but not interactively, and the equations of Model II adequately represent activity dynamics.

*Model II formulas.* The Model II formulas for predicting activity resultants are as follows.

$$S' = .16 + .79S + .29V + .26Q \quad [9]$$

$$V' = .02 + .44S + .41V + .31Q \quad [10]$$

$$Q' = .06 + .37S + .18V + .43Q \quad [11]$$

Again, it appears feasible to develop a formula for a general sentence resultant and another formula for predicting the resultant for any particular word in the sentence. The actual equations, with numerical estimates of the parameters, are as follows.

$$R = .09 + .45S + .24V + .25Q \quad [12]$$

$$W' = R + .25W \quad [13]$$

The latter equation accounts for 68% of the variance in activity resultants, whereas the mean proportion of variance accounted for by using Model II separately for each set of components is .69.

### *Potency*

The desired sampling design for potency sentences failed to materialize in actuality. When the average original potency ratings were calculated, it was found that none of the sentences had a distinct +− or −+ pattern in the *V*-*Q* positions. Thus, the potency data do not contain as much meaningful variance as previous sets of data, and they are not representative of the total possible range of sentences. The attenuation of variance leads to attenuation of correlations, so the potency results, presented in Table 5, are not as impressive as those for evaluation and activity.

TABLE 5

Summary Statistics ($R^2$) from Multiple Regressions Based on Various Models for Predicting Potency Resultants

| Dependent variable | Model I | Model II | Model III | Model IV |
|---|---|---|---|---|
| $S'$ | .08 | .31 | .36 | .42 |
| $V'$ | .35* | .42* | .44* | .55* |
| $Q'$ | .00 | .20 | .46* | .48 |

* Regression coefficient statistically different from zero, $p < .05$.

Regressions based on Model I are notable for predicting *V'* fairly well, but not at all notable for predicting *Q'*. This suggests that in potency dynamics, the *V* remains fairly stable, and *Q* very unstable.

Although the obtained levels of prediction still are not extremely high with Model II, the model does appear to be an improvement over Model I. The proportions of explained variance increase by .23 for *S'*, .07 for *V'*, and .20 for *Q'*.

Model III, which adds a $V \times Q$ interaction to Model II, appears to afford little gain in prediction efficiency for *S'* and *V'*, but it increases the explained variance in *Q'* by 26 percentage points. Including all interactions in predictions (Model IV) seems to yield only minor gains in predictive power for the *S* and *Q*. $R^2$ increases by .06 for *S'* and by .02 for *Q'*. However, the additional terms give a small but noticeable increase of .11 units in $R^2$ for the *V'*s.

These results indicate that potency dynamics may be very complex. Subject resultants were predicted fairly adequately using Model II with no interactions; prediction of object resultants seemed to require a $V \times Q$ interaction term; and *V'*s were mainly a function of original *V* ratings, although some variations appeared to be a function of complex interactions.

## Discussion

### *Evaluation Model*

The linguistic model for analyzing evaluation dynamics fits well with traditional attitude-balance theories. In particular, the Verb × Object interaction term yields the same kind of "peculiar" predictions when considering negative attitudes. For example, while beggars, rapists, and slugging are all bad, a beggar who slugs a rapist is raised in esteem because of the strong interaction between verb and object. Perhaps the most notable difference between the linguistic model and traditional balance theories is in the level of quantification. The essence of the linguistic model is a mathematical formula (Formula 8) that can be applied to attitude measurements for prediction purposes, and results in the present study, and in Gollob's (1968), indicate that predictions using such a formula are quite accurate.

The mathematical formulation reveals some aspects of attitude balance that are not especially evident in verbal theories. First, dissociation from bad objects does not always result in positive evaluation toward the subject. In fact, a subject does *not* improve an evaluation by being dissociated from a disliked object if: (*a*) the verb is too intensely negative relative to the object. Verb attitudes have a direct additive impact on the resultant which can counteract the Verb × Object interaction. Thus, in personal terms, if a person acts too foul he may debase himself regardless of the object of his action; (*b*) the object is only slightly disliked. If an object attitude is only slightly negative, a major Verb × Object interaction cannot be built up to counterbalance the deevaluation originating in the negative verb. Therefore, negative acts yield positive evaluation only if directed at objects that are fairly reprehensible.

On the other hand, the formula indicates that a positive association with bad objects does not necessarily result in negative evaluation. It appears that the direct impact of a positive verb—or of positive actions—is so strong in attitude dynamics that it counteracts the Verb × Object interaction when the object is only mildly disvalued.

Examination of the evaluation-resultant formula reveals that there should be two special instances in which the value of a sentence resultant is a function of the subject only. One of these is when the verb is completely neutral ($V = 0$). Under this condition, an object can be chosen that is valued positively or negatively to any degree, and the resultant is not affected. Translated to personal terms, this means that if one acts in a completely neutral manner, it is possible to deal with any object without degrading oneself. The principle seems to have relevance for interpreting the basic codes of conduct and the development of neutralized jargon in law, science, and the healing professions. For example, a judge's reputation might suffer if he "listened to" a murderer, but the judge is unsullied if he "hears" him.

The same kind of condition should occur when the object has a certain negative value: using the parameter estimates in the resultant formula, the peculiar value of $Q$ is $-.44/.28 = -1.57$. Theoretically, any verb at all can be used with such objects without affecting the evaluation resultant. This is an unexpected finding, and it gives rise to a new hypothesis: some negatively valued objects exist that may be liked or disliked, nurtured or exterminated, with the attitudinal consequences of all these acts depending only on the initial evaluation of the subject engaging in the action.

It might be noted that the linguistic model for attitude dynamics assumes cognitive processes at several points. First, cognitive factors are involved in the definition of a situation. For example, it makes a difference attitudinally whether one thinks "the police beat up the kids" or "the police beat up the hooligans." In the model, definition of the situation is essentially a matter of selecting among alternative words. Cognitive factors also are involved in the semantic interpretation of assertions. For example, "he destroyed his pot" can create different impressions depending on whether "pot" is interpreted as a container or a drug. The decoding process determines which attitude is entered into dynamic processes. Finally, cognitive factors are involved in syntactical processing of attitudes. For example, "the police beat up the hooligans" is different than "the hooligans beat up the police," and, in general, recognizing the grammatical positioning of words is of critical importance for attitudinal dynamics.

### *Activity and Potency Dynamics*

Evaluation appears to be the dominant mode of affective response, so attention has focused primarily on evaluation dynamics. However, numerous studies involving the semantic differential indicate that at least two other dimensions of affective response exist, and thus an effort was made in the present study to examine the dynamics of activity and potency reactions.

Impressions of activity combine in sentences in a simple additive manner. A subject word that normally has associations of activity tends to create an overall impression of activeness when entered in an assertion, and lively verbs and objects have the same effect. Similarly, inactive subjects, verbs, or objects contribute toward a net impression of stillness. This finding indicates, first, that it is sensible to talk about activity dynamics in sentences: the activity connotations of words do change when the words are combined in assertions. The finding also indicates, however, that activity dynamics are relatively simple. The activity resultant for a sentence is merely an average of the activity associations for the separate words, and there are no interactions to produce peculiar changes, as in the case of evaluation dynamics.

The weighting constants in the formula for activity resultants reveal two interesting properties of activity dynamics. First, while the $V$ contributes to the resultant, it does not have a substantially greater impact than the $S$ or the $Q$. This eliminates a possible common-sense postulate about activity dynamics—that the character of the action is the crucial variable determining activity impressions. Second, the $S$ does have considerably more impact on the resultant than either the $V$ or the $Q$. This could be a primacy effect (the $S$ always appeared first in the sentences studied), but assuming for now that the effect is not merely due to primacy, it indicates that in transitive sentences the source of action is a key determinant of activity impressions. This can be interpreted as a relation between the $S$ and $Q$: the $S$ affects the $Q$ more than vice versa, and phrasing in terms of interpersonal events, the liveliness or quietness of an actor is contagious to those he acts upon.

The results dealing with potency connotations suggest that potency dynamics may be exceptionally complicated, with a different resultant formula required for each term in a sentence. The $S'$ appears to be a simple additive function of original associations, the $V'$ appears to be relatively unaffected by sentence dynamics, and the $Q'$ seems to be mainly a function of a $V \times Q$ interaction that operates such that a powerful act on a potent $Q$ makes the $Q$ stronger while a powerful act on a weak $Q$ makes the $Q$ more impotent. Another study will have to be conducted to clarify potency dynamics, since the results here were not unequivocal. It might be noted that the topic is of particular interest because a translation of potency dynamics into the psychology of dominance relations seems possible.

## *A Theoretical Extension*

A major current of recent thought in social psychology has been that attitudes have motivating power when they produce tension by being associated in incongruent ways. Let psychological tension be defined as the total pressure for affectual modification that arises out of a particular sentence or event. Assuming the pressure is directly proportional to the actual amount of change that can be expected, the tension due to evaluation dynamics ($T_E$) would be:

$$T_E = |S-S'| + |V-V'| + |Q-Q'| \quad [14]$$

Similarly, there would be tension corresponding to expected changes in activity and potency, and the total tension evoked by a sentence or event then would be the sum, $T_E + T_A + T_P$.

The basic postulate in tension theory is that persons strive to minimize tension. This, however, can be done through a variety of tactics: attitude change, direct action, redefinition of the situation, withdrawal, psychological defenses, etc. Although different tactics of tension reduction exist, one would expect that, in a population, the probability of any tactic appearing should increase when persons are confronted with high-tension sentences or events. Thus, it should be possible to use the formulas presented in the present report to design sentences that evoke defensive tactics, and thereby one is in a position to develop quantitative, controlled experiments dealing with tension theory.

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# EFFECTS OF ANTICIPATING FUTURE INTERACTION ON THE PREFERENCE FOR BALANCED STATES

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Previous balance studies in which subjects are asked to identify with the hypothetical person, P, have repeatedly found, contrary to Heider's hypothesis, that balanced situations containing a negative interpersonal bond are rated as no more pleasant than imbalanced situations. It was hypothesized that the lack of preference for these balanced negative-bond situations stemmed from the subjects' implicitly assuming that there would be future interaction with the other person and that if such an assumption were explicitly refuted, preference would be expressed for all balanced situations. Two hundred sixty-eight subjects rated each of eight hypothetical situations involving themselves, another person, O, and an impersonal entity, X, for experienced pleasantness. One-fourth of the subjects received only task instructions. The other subjects were additionally instructed to assume (*a*) future contact with O concerning X, (*b*) future contact with O but not concerning X, or (*c*) no future contact with O. The ratings made within and across assumption conditions supported the hypotheses and suggested that the applicability of the balance principle may be temporally restricted. The interrelationship of involvement, self-concern, and anticipating the future was briefly discussed.

One of the basic principles in Heider's balance theory is that there exists a preference for balanced states (1958, pp. 204–205). The bulk of evidence bearing on this assumption comes from ratings of the pleasantness of hypothetical situations involving two persons (P and O) and an impersonal entity (X). As described by Heider, "A triad is balanced when all three of the relations are positive or when two of the relations are negative and one is positive [1958, p. 202]." Thus, where P likes O, the situation is balanced when P and O have the same attitude (positive or negative) toward X, and imbalanced when they disagree regarding X. Where P dislikes O, balance results from disagreement regarding X, and imbalance from agreement.

As extensively reviewed by Zajonc (1968), the investigations testing the validity of the preference assumption have yielded apparently inconsistent results. Studies in which subjects are asked to play the role of P have generally found that balanced situations which include a positive P-to-O bond are rated significantly more pleasant than imbalanced situations. However, contrary to Heider's formulation, balanced situations which contain the specification that P dislikes O are rated no more pleasant than imbalanced situations by the subjects asked to take the role of P, that is, who are "P-identified" (e.g., Hershkowitz, 1954; Jordan, 1953; Price, Harburg, & Newcomb, 1966; Rodrigues, 1965, 1967). Better support for balance theory, on the other hand, comes from studies in which the subject deals with hypothetical situations in which he is not supposed to be involved. In this case, researchers (Steiner & Spaulding, 1966; Zajonc & Burnstein, 1965a, 1965b) have obtained balance effects not only for the P-likes-O cases, but also for the P-dislikes-O cases.

The unpleasantness ratings that P-identified subjects give to balanced P-dislikes-O situations have been explained by postulating other cognitive biases, such as preferences for *positivity* of the feeling between P and O, and for *agreement* between P and O regarding X. In situations where P likes O, the preferences for balance, interpersonal positivity, and agreement all work in the same direction. However, in situations where P dislikes O, the preferences for interpersonal positivity and agreement work in opposition to the forces toward balance. Supposedly, the P-identified subjects' ratings are affected by this conflict of forces to the extent that the balanced P-dislikes-O situations are experienced as unpleasant.

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[1] The author would like to express his appreciation to Leonard Berkowitz for his advice and critical comments. Requests for reprints should be sent to David Aderman, Department of Psychology, University of Wisconsin, Madison, Wisconsin 53706

The validity of this explanation is called into question by the findings of the previously mentioned studies in which subjects are not asked to identify themselves with P. Despite evidence that preferences for interpersonal positivity and agreement were operative, these studies obtained strong support for the balance principle. How then can one account for the failure of P-identified subjects to rate balanced P-dislikes-O cases as pleasant? The discrepancy in findings between balance studies requiring subjects to involve themselves in the hypothetical situations and those having no such requirements raises the possibility that the P-identified and nonidentified subjects could conceivably be making different assumptions about the situations presented to them and/or might be experiencing these situations differently.

An experiment by Stotland and Sherman (cited in Stotland, 1969) provides a clue as to how the experience of imagining oneself in a situation (the P-identification requirement) would differ from the experience of imagining someone else in the same situation (the nonidentification requirement). All subjects in their study observed a fellow subject (actually a stooge) receive an apparent diathermy treatment. One group of subjects (imagine-self condition) was instructed to imagine how they themselves would feel if they were receiving the treatment. Another group of subjects (imagine-him condition) was to imagine how the actual recipient felt during the treatment. Comparison of the vasoconstriction and palmar sweating reactions of these two groups of subjects indicates that the reactions of the imagine-him subjects were restricted to the onset of the diathermic treatment. Stotland and Sherman suggested that these subjects were reacting to feelings they perceived the recipient as having *at a given moment*. In contrast, the reactions of the imagine-self subjects were less time-bound, and less tied to the recipient's immediate experience. In fact, when the recipient was to receive a painful treatment, the imagine-self subjects reported a high level of negative affect both before and after the machine was turned on.

The Stotland and Sherman study suggests the possibility that the perception of the nonidentified subjects in balance experiments is restricted to the hypothetical situations as they presently exist, while the perception of P-identified subjects may incorporate the extension of the existing situations into the future, thus creating the anticipation of future contact with O concerning X. The present study inquires as to the likelihood and consequences of P-identified subjects' assuming that there will be future contact with O concerning X.

In situations where O is disliked, the anticipation of future contact with O is likely to be unpleasant. Furthermore, in situations where P and O disagree regarding X, the anticipation of further discussion of X is also likely to be unpleasant. In those situations where O is disliked *and* P and O disagree about X, that is, the balanced P-dislikes-O cases, the assumption that there will be future contact with O concerning X would be expected to generate unpleasant anticipations with respect to both future contact and further discussion. The unpleasantness associated with the anticipation of unwelcome interaction could be what is responsible for P-identified subjects expressing little or no preference for the balanced P-dislikes-O cases.

If a sufficient number of P-identified subjects do make the implicit assumption that there will be future contact with O concerning X, the consequences of making this assumption explicit should be minimal. The ratings obtained from subjects explicitly instructed to make such an assumption should be similar to the ratings made by P-identified subjects given only standard task instructions. Moreover, if subjects were instructed to assume that there definitely will not be future contact with O, thus explicitly refuting their implicit assumption, their ratings of balanced situations should be more pleasant than their ratings of imbalanced situations regardless of the P-to-O bond sign.

The present balance study, requiring all subjects to identify with P, was designed as a test of the above reasoning. One group of subjects (the replication condition) received only standard task instructions. A second group of subjects was instructed, in addition, to assume that there would be future contact with O concerning X. It was expected that the ratings of both groups would match those obtained in previous studies which required subjects to identify with P, in that only balanced situations where P-to-O was positive would be rated as pleasant. A third group of subjects was additionally told to assume that there definitely would not be future contact with O. It was hypothesized that under this assumption subjects would express preference for all the balanced situations. Finally, a fourth group of subjects received the additional instruction to assume that there would be future contact with O, but that X would not be discussed. This assumption was included in order to make it possible to ascertain when the anticipations of future contact and further discussion would be experienced unpleasantly. The effect of ruling out the possibility of further discussion of X, it was felt, would be to lessen the importance of the P-to-X and O-to-X bonds, and heighten the importance of the P-to-O bond sign. It was expected, therefore, that all the P-likes-O cases would be rated as more pleasant than the P-dislikes-O cases.

## METHOD

### *Subjects*

The subjects were 268 undergraduates enrolled in either introductory general psychology or introductory social psychology at the University of Wisconsin. The sample was composed of nearly equal numbers of males and females and of nearly equal numbers of students from each course. Sixty-seven subjects were randomly assigned to each of the four experimental conditions.

### *Procedure*

The experiment was performed during the third week of classes at a regular meeting of the social psychology course, and in various quiz sections of the introductory psychology course. Material dealing with the various consistency theories had not been covered in either course up to the time of the experiment. Each subject received a booklet of stapled sheets consisting of an instructions page and eight task pages. The instructions read as follows:

> On each of the following eight pages is a description of a situation involving two persons, "I" and "O," and an impersonal entity, "X." *Imagine yourself* playing the role of "I," and then rate the situation for experienced pleasantness or unpleasantness by crossing the rating line at a point corresponding to how you think "I" would feel.

TABLE 1

ANALYSIS OF VARIANCE OF UNPLEASANTNESS SCORES

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Assumptions (A) | 3 | 2,616.48 | 6.21* |
| Error between | 264 | 421.41 | |
| Situations (D) | 7 | 35,634.12 | 94.72* |
| A × D | 21 | 3,977.21 | 10.57* |
| Error within | 1,848 | 376.22 | |

* $p < .001$.

The group of subjects randomly assigned to the replication condition received only these instructions. For the three groups of subjects randomly assigned to the future contact assumption conditions, the *same* assumption about future contact between I and O was specified on each task page beneath the described situation and above the rating line. Each of these subjects was presented with *only one* of the following three assumptions: (*a*) I and O will definitely have future contact and will discuss X, (*b*) I and O will definitely have future contact but will *not* discuss X, and (*c*) I and O will definitely *not* have future contact.

The eight task sheets presented the subjects with the eight different hypothetical situations (e.g., "I dislike O"; "I like X"; "O dislikes X"). Each situation appeared with approximately equal frequency in each position of the sequence of presentation.

In keeping with the practice originated by Jordan (1953), the subjects' responses were scored by placing a millimeter ruler along each rating line, and the score corresponded to the mark on the ruler indicated by the subject's response. The lowest point ("most pleasant situation") was equal to 0, the middle point ("neutral situation") was 45, and the highest ("most unpleasant situation"), 90 millimeters.

## RESULTS

The analysis of variance of the unpleasantness ratings is presented in Table 1. No significant main effects or interactions were associated with sex or course group of subjects, and the analysis is collapsed over these factors. The significant Assumptions × Situations interaction indicates that the various future contact assumptions produced differential effects on the subjects' responses to the hypothetical situations. The mean ratings of unpleasantness for each of the eight situations under the four different assumption conditions are presented in Table 2.

The mean ratings of unpleasantness made by subjects given only standard instructions

TABLE 2

MEAN RATINGS OF UNPLEASANTNESS FOR BALANCED AND IMBALANCED SITUATIONS UNDER DIFFERENT FUTURE CONTACT ASSUMPTIONS

| Assumption | Balanced | | | | Imbalanced | | | |
|---|---|---|---|---|---|---|---|---|
| | +++[a] | +−− | −+− | −−+ | ++− | +−+ | −++ | −−− |
| 1. Standard instructions only | $13.18_{a, A}$ | $27.51_{b, A}$ | $47.29_{c, B}$ | $55.86_{c, B}$ | $51.22_{c, B}$ | $54.74_{c, B}$ | $52.02_{c, B}$ | $49.85_{c, AB}$ |
| 2. Future contact and discussion of X | $18.48_{a, AB}$ | $30.36_{b, A}$ | $63.60_{d, D}$ | $66.77_{d, C}$ | $52.54_{c, B}$ | $50.06_{c, B}$ | $52.88_{c, B}$ | $53.73_{c, AB}$ |
| 3. Future contact but no discussion of X | $25.13_{a, B}$ | $27.98_{a, A}$ | $57.23_{c, CD}$ | $57.86_{c, B}$ | $40.07_{b, A}$ | $38.91_{b, A}$ | $55.12_{c, B}$ | $57.24_{c, B}$ |
| 4. No future contact | $33.92_{a, C}$ | $39.22_{ab, B}$ | $37.49_{ab, A}$ | $42.99_{bc, A}$ | $50.51_{cd, B}$ | $54.39_{d, B}$ | $43.03_{bc, A}$ | $47.79_{cd, A}$ |

Note.—Lowercase subscripts apply only to comparisons made within the same row. Uppercase subscripts apply only to comparisons made within the same column. The subscripts have been assigned in descending order of pleasantness; that is, A or a indicates most pleasant rating and D or d indicates least pleasant rating. Cells which share a common subscript are not significantly different from each other at the .05 level by the Newman-Keuls test.

[a] The first sign refers to the I-to-O bond, the second to the I-to-X bond, and the third O-to-X bond.

successfully replicated those obtained in the previous balance studies requiring subjects to identify with P. Only the two balanced situations which contain a positive I-to-O bond were rated as pleasant. The other two balanced situations were rated just as unpleasant as the four imbalanced situations.

In line with expectations, instructing subjects to assume that there would be future contact with O concerning X did not greatly alter the pattern of responding described above. The only significant consequence of making explicit the assumption of future contact was to increase the reported unpleasantness of balanced I-dislike-O cases to the extent that subjects rated them as even more unpleasant than the imbalanced situations.

The ratings of subjects who were instructed to assume that there would be future contact with O but not concerning X tended to reflect, as expected, the sign of the I-to-O bond. While the imbalanced I-like-O cases were rated as more unpleasant than the balanced I-like-O cases, they were also rated as significantly more pleasant than any of the I-dislike-O situations.

It was hypothesized that subjects rating under the explicit assumption that there would be no future contact with O would demonstrate a preference for all balanced situations. The data only partially confirm this hypothesis. One of the two balanced I-dislike-O situations (−+−) was significantly preferred to three of the four imbalanced situations, but the other (−−+) was significantly preferred to only one of the four imbalanced situations. Using Scheffé's (1953) conservative test for multiple comparisons, the average pleasantness of the four balanced situations was found to be higher than the average pleasantness of the four imbalanced situations at beyond the .001 level of significance. This finding cannot be attributed solely to the pleasantness of the two balanced I-like-O situations, since the average pleasantness of the two balanced I-dislike-O situations was also higher ($p < .05$) than the average pleasantness of the four imbalanced situations. It should be pointed out, however, that the mean value for the two balanced I-dislike-O situations does not differ significantly from the mean value of the two imbalanced I-dislike-O situations.

The equivocal results yielded by these individual and multiple comparisons may be attributable to the "neutralizing" effect the assumption of no future contact appears to have had on the ratings. As inspection of Table 2 reveals, for seven of the eight situations, the pleasantness ratings under "no future contact" are nearer to 45, the midpoint, than under "standard instructions only." This "neutralizing" effect could be considered simply regressive, or what is more in keeping with the outcome of comparisons made across assumption conditions, as an artifact of the differential reactions to the anticipation of no future contact in I-like-O and I-dislike-O situations. Whatever its source, because of the "neutralizing" effect of assuming no future contact, perhaps a fairer test of the hypothesized preference for the two balanced I-dislike-O situations under the assumption of no future contact would result from expressing the difference in average pleasantness of the balanced I-dislike-O and I-like-O situations as a *proportion* of the difference in average pleasantness of the balanced I-like-O situations and the four imbalanced situations. In contrast with proportions of .99 under "standard instructions only," and 1.46 under each of the future contact assumptions, this proportion, in support of the hypothesis, is only .30 under "no future contact."

The comparisons made across assumption conditions (the uppercase subscripts in Table 2) confirm, first of all, that it is unpleasant to anticipate future contact with a disliked other. The ratings of three of the four I-dislike-O situations were significantly more pleasant under the no future contact assumption than under either of the future contact assumptions (or under "standard instructions only"). The (− − −) triad was rated significantly more pleasant under "no future contact" than under "future contact but no discussion of X," but only nonsignificantly more pleasant than under "future contact and discussion of X" or "standard instructions only." This was the only instance within the present study consistent with Heider's (1958) contention that the case of three negative relations is "somewhat ambiguous [p. 203]."

The comparisons made across the two future contact conditions indicate, moreover, that it is unpleasant to anticipate discussion of X when there exists disagreement with O concerning X. Finally, the comparisons reveal that if a person agrees with someone he likes, then it is more pleasant to anticipate future contact than to anticipate no future contact. However, if a person disagrees with someone he likes, the anticipation of future contact is not perceived as any more pleasant than the anticipation of no future contact, provided there is also anticipation that the topic of disagreement will be discussed.

## DISCUSSION

The present study indirectly tested the likelihood of P-identified subjects actually assuming future contact with O concerning X. The generally close correspondence between the ratings made by subjects given only standard instructions and those explicitly instructed to assume future contact with O concerning X makes it reasonable to assert that a significant number of P-identified subjects do implicitly assume such contact. The fact that the group of subjects given only standard instructions did not rate the balanced I-dislike-O situations as more unpleasant than the imbalanced situations, while the subjects given the explicit assumption did, indicates that not all the subjects implicitly assumed that there would be future contact with O concerning X. Some subjects may have anticipated the absence of future contact with O, particularly in the balanced I-dislike-O situations. It is likely that still other subjects made no implicit assumptions whatsoever concerning the future.

Balance-favoring ratings were obtained only under the explicit assumption of no future contact. This suggests that the applicability of the principle of balance may be restricted to the perception of interpersonal relations *at a given moment in time*. It appears, however, that the perception of subjects who are required to involve themselves in the hypothetical situations is not similarly restricted. It might reasonably be conjectured that requiring subjects to imagine themselves in the hypothetical situations heightened their self-concern in the sense that they became more attentive to factors that were capable of producing consequences for them. Considering the extension of interpersonal relations into the future would permit such a self-concerned individual to anticipate the outcomes that future interaction would provide for him, so that, if afforded the opportunity, he could take steps toward insuring that the outcomes be favorable (e.g., change his attitude toward X; perceive O more favorably). The P-identified subjects in the present study were not provided a means to insure favorable outcomes. They could only register satisfaction or dissatisfaction with the outcomes they anticipated experiencing. The comparisons made across assumption conditions confirm that this is, indeed, what the subjects did.

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# ATTITUDE, OBJECT LABEL, AND STIMULUS FACTORS IN RESPONSE TO AN ATTITUDE OBJECT[1]

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The present study examines the hypothesis that the relationship between a person's attitude and his reaction to an attitudinal object depends upon the label attached to that object. If attitudes are based on motives that are not tied directly to the characteristics of the object, then the correspondence between an independently measured attitude toward an object not easily identified by superficial cues and reaction to the object should be substantially less than if the object when confronted carries its attitudinal label. One hundred forty-five subjects systematically sampled from three communities were studied using the community drinking water as the attitudinal object. Results support the hypothesis and are discussed in terms of object characteristics and nonobject, attitudinal factors as determinants of response to attitude objects.

Current attitude theories hold that most attitudes are in some degree based upon factors other than the actual characteristics of the attitude object. For example, Smith, Bruner, and White (1956) identify several motivational bases for attitudes, the principal ones being socialization and externalization, as well as assessment of the object. Katz and Stotland (1959) and Kelman (1961) also cite major influences other than the features of the object itself. These theoretical points of view, which have some empirical support (e.g., see Katz, 1960), permit the possibility that the actual characteristics of the object itself may be of less importance than other factors in determining response to the attitude object.

The purpose of the present experiment was to investigate the relationship between attitude and response to an attitude object not superficially identifiable when the characteristics of the attitude object are held constant and the label is independently manipulated. To the extent that the attitudinal stimuli are objective characteristics of the object, the presence or absence of the attitudinal label on the object should be of little consequence; confrontation with the object should induce the label as well as the attitude and yield the same response whether or not the label is supplied. If, however, the label is a significant portion of the stimulus complex and is not induced by characteristics of the object, then response to the object should be different when the label is appended than when it is not. The hypothesis underlying the present research is that response to an attitude object that does not carry readily identifiable superficial cues that induce the label shows greater correspondence to attitude as independently measured when the label is included in the stimulus complex.

## METHOD

The attitude object studied was the subjects' community drinking water, an object that makes possible independent manipulation of the label of the object and meets the condition of the subjects' considerable direct experience with the stimulus object itself. The latter condition sets a stringent test of the hypothesis since it provides ample opportunity for an object-based attitude. One of the criteria for selecting communities for study was that there be

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little or no variation from sample to sample of a given community water supply. It was thus possible to control variation in reaction to the specific attitude objects due to sampling variation while still using actual samples. In addition to these conditions, the community drinking water in two of the three communities studied represents a topic of concern because of its quality characteristics (Bruvold, Ongerth, & Dillehay, 1967). Common minerals were judged to be the only significant taste-producing constituent in each water supply. Total common mineral content in the communities was 50 milligrams per liter, 388 milligrams per liter, and 1,194 milligrams per liter. In general, more unfavorable attitudes are associated with higher mineral content in drinking water (Dillehay, Bruvold, & Siegel, 1967). The communities are in effect replications for the present study and are pooled for the analyses.

### *Subjects*

Subjects were selected by listing all occupied dwelling units in each area under study, selecting approximately 50 dwelling units from each area by random-start systematic sampling, censusing each selected dwelling unit, and randomly selecting approximately 50 English-reading subjects 15 years of age or older from the pool of subjects for each community. Each subject was sent a letter about 2 weeks before the study, and the general purpose of the project was explained as a study of what people think of different kinds of drinking water. Of those contacted, 94% participated in the study ($N = 145$).

### *Instruments*

Three scales were used to assess attitudes toward the community drinking water; the construction of these scales and their characteristics are reported elsewhere (Dillehay et al., 1967). The data reported in the present paper are based on only one scale since the intercorrelations among the scales ranged from .89 to .94. The scale is a Thurstone-type instrument dealing with evaluation of the water specific to taste quality. The scales were administered in the usual manner: subjects were asked to consider their own community drinking water and check those attitude statements that applied. The higher the score, the more favorable the attitude.

The measures of response to the attitude objects themselves were obtained by having subjects taste water samples from their own community interspersed among additional water samples. Responses to the actual water samples were obtained using a rating scale consisting of 14 categories with each category described by a statement of evaluation and a statement of usage intention (Bruvold & Pangborn, 1966). Ratings could range from negative evaluation coupled with a definite refusal to use the water for drinking (Category 14) to positive evaluation and a statement of free willingness to drink the water (Category 1). The neutral point on the scale is about Step 3 since the purpose of the instrument was to distinguish better among shades of negative evaluation. The ratings were converted so that high scores indicate favorable reaction to the water samples.

### *Procedure*

The data were collected in laboratory settings in each of the communities. Appointment times were set at the convenience of the subjects. Each subject was first interviewed following a standardized procedure designed to yield information about the subject's general feelings about the water, his use of it, what others think about it, its cost, the cost of alternatives, how long he lived in the community, water quality of other localities as he saw it, and socioeconomic level. After the interview was completed, the attitude scales were administered.

Subjects then were given a rating task involving 24 water samples in which the subject was to taste each sample and assign it a rating on the 14-point scale. Four of the water samples were from the subject's community water supply. The remaining 10 samples, each responded to twice in the procedure, were 1,000-milligram-per-liter solutions of $NaHCO_3$, $Na_2SO_4$, $CaSO_4$, $MgSO_4$, NaCl, $CaCl_2$, $MgCl_2$, and $Na_2CO_3$, distilled water, and Berkeley tap water (which contains approximately 85 milligrams per liter of total dissolved solids). The water samples were rated in two blocks of 12 samples each. Order was fully randomized for Block 1. Eleven samples of Block 2 were randomly ordered. The twelfth sample of Block 2 was always a clearly identified sample of the local community water. All other samples were presented in coded beakers. Two coded samples of community water were presented in Block 1, and one coded sample was presented in Block 2. No subject gave any indication of recognizing the coded samples of his community water. The mean of the ratings of these three samples was taken as a measure of response to the unlabeled attitude object.

In all, the entire procedure took about 1 hour and 15 minutes; the rating task required about 50 minutes. A more detailed discussion of all procedures is given by Bruvold (1965).

## RESULTS AND DISCUSSION

Data analyses yielded the following correlations among the attitude scale and reactions to the water samples: attitude scale versus reactions to the unidentified water samples, .19; attitude scale versus response to the identified water samples, .68; reaction to the identified water samples versus reaction to the unidentified water samples, .36. In each case the correlation is significantly different from zero. When tested by the Hotelling procedure (Walker & Lev, 1953), the correlation of .19 is significantly different from that of .68.

These relationships offer clear support for the hypothesis. When the attitude object is labeled so that both the object and its label comprise the stimulus, the correlation between attitude as independently measured and response to the actual object is significantly higher than when the same attitude object does not carry the attitude label.

If we accept the assumption that people try to be consistent in their evaluations of the symbolic presentation of an attitudinal object and reactions to the actual object, then the high relationship between attitude as separately assessed and response to the identified object is not surprising. The same disposition toward consistency should operate, however, when the object is confronted without its attitudinal label, limited only by the extent to which the actual object characteristics do not conform to the stereotype of the object. A significantly different response to the object without its attitudinal label indicated the extent to which the attitude is based on factors other than the object itself.

The influence of the characteristics of the object upon the relationship between attitudes and reaction to the object can be assessed by a partial correlation analysis. If one regards the correlation between attitude as separately measured and reaction to the unlabeled object as reflecting the extent to which the attitude is based on the characteristics of the object itself as contrasted with other determinants of attitude, and if one considers the relationship between the responses to the labeled object and reaction to the unlabeled object to reflect the extent to which the responses to the labeled object are objectively based, then one may obtain an indication of the extent to which factors other than those located in the object are determining attitudinal response to the labeled object. The indication operationally is the correlation between the attitude and the labeled object with the attitude–unlabeled-object relationship partialed out. This correlation will be much reduced if object-tied factors are a considerable influence in the relationship between attitude and the labeled object; the correlation will not be much reduced if substantial factors other than those tied to characteristics of the stimulus object are influential. Using the present data, the partial correlation between attitude and the labeled object is .67, down only slightly from .68. A substantial amount of the relationship between the attitude and the labeled object can thus be attributed to the stimulus importance of the label quite apart from the characteristics of the object itself.

The present study provides an empirical basis for an important factor in the consideration of the relationship between attitudes and behavior—the differential between the stereotype and the object itself. To the extent that there is a discrepancy between the stereotype and the object, attitudes as conventionally measured will fail of predicting behavior. It is not a matter of invalidity of attitude assessment; the point is rather that when the object is symbolically presented for attitude assessment (as with a descriptive term or phrase) the object of the attitudinal response is the stereotype. If the stereotype is not engaged by the object when it is encountered, then the correspondence between attitudes and behavior should be reduced. Future research might deal with characteristics of the attitude object (such as complexity and ambiguity) and extra-object factors (e.g., pressures toward consensus) that should influence the correspondence between attitudinal object and stereotype.

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## Manuscripts Accepted for Publication in the *Journal of Personality and Social Psychology*

Immediate and Long-Term Effects of Experimentally Induced Social Influence in the Modification of Adolescents' Moral Judgments: William G. LeFurgy* and Gerald W. Woloshin: Department of Psychology, Grant Hall, Princeton University, Princeton, New Jersey 08540.

Model, Need, and Cost Effects in Helping Behavior: Carl Wagner* and Ladd Wheeler: Naval Medical Research Institute, National Naval Medical Center, Bethesda, Maryland 20014.

Opinion Change in the Advocate as a Function of the Persuasability of His Audience: A Clarification of the Meaning of Dissonance: Elizabeth Nel, Robert Helmreich,* and Elliot Aronson: Department of Psychology, Mezes Hall 221, University of Texas, Austin, Texas 78712.

The Group as a "Polarizer" of Attitudes: Serge Moscovici* and Marisa Zavalloni: Center for Advanced Study in the Behavioral Sciences, 202 Junipero Serra Boulevard, Stanford, California 94305.

Intergroup Relations and Polarization of Individual and Collective Judgments: William Doise*: c/o Serge Moscovici, Center for Advanced Study in the Behavioral Sciences, 202 Junipero Serra Boulevard, Stanford, California 94305.

Individual versus Triadic Performance on an Unidimensional Complementary Task as a Function of Initial Ability Level: Patrick R. Laughlin,* Laurence G. Branch, and Homer H. Johnson: Department of Psychology, Loyola University, 6525 North Sheridan Road, Chicago, Illinois 60626.

Status and Sex of Other as Determinants of Behavior in a Mixed-Motive Game: Malcolm J. Grant* and Vello Sermat: 150 Snowdon Avenue, Toronto 12, Ontario, Canada.

Psychic Cost of Adaptation to an Environmental Stressor: David C. Glass,* Jerome E. Singer, and Lucy N. Friedman: Research Center for Human Relations, New York University, 4 Washington Place, 6th Floor, New York, New York, 10003.

Tonic Heart Rate: Experiments on the Effects of Collative Variables Leading to an Hypothesis about Its Motivational Significance: Elliott Rogers*: Department of Psychology, Dartmouth College, Hanover, New Hampshire 03755.

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# EFFECTS OF PRIOR HELP AND HINDRANCE ON WILLINGNESS TO HELP ANOTHER:

## RECIPROCITY OR SOCIAL RESPONSIBILITY[1]

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An experiment using 72 young female undergraduates examined the effects of prior help on subsequent help giving. An ostensible peer (*P*1) gave one-third of the subjects help in winning a prize. Another one-third of the subjects received no help, and the remaining subjects were hindered. Another person (*P*2) needing the subject's help was represented to one-half of the subjects as similar to *P*1 and to the remaining subjects as dissimilar to *P*1. Prior help led to high help for *P*2 regardless of *P*1–*P*2 similarity. Prior hindrance resulted in hindrance for a similar *P*2 and help for a dissimilar *P*2. It was concluded that while a subject's help for *P*2 in the prior help condition was a function of a new norm of social responsibility, a subject's hindrance of a similar *P*2 in the prior hindrance condition was due to either the negative norm of reciprocity or to a generalization of resentment. The relatively high amount of help that the subject gave to a dissimilar *P*2 after receiving hindrance was attributed to the subject's exaggerated awareness of the normative requirements of the situation prescribing help.

Recent research suggests that behavior in certain situations is governed by a norm of social responsibility which prescribes that people should help those needing it even though the beneficiaries (or their surrogates) had not helped them earlier and may not provide repayment in the future (Berkowitz & Daniels, 1963; Berkowitz, Klanderman, & Harris, 1964; Daniels & Berkowitz, 1963; Goranson & Berkowitz, 1966). Further research has demonstrated that after prior help, subjects gave greater help to a dependent person than did subjects who received no prior help (Berkowitz & Daniels, 1964; Goranson & Berkowitz, 1966). Berkowitz and Daniels (1964) suggested that prior help from one person may have increased help for another person because it increased the salience of the social responsibility norm, thereby reminding the subject to help a dependent person.

One explanation for the results reported by Berkowitz and Daniels (1964) involves the norm of reciprocity which prescribes that people should give benefits to those from whom benefits have been received previously, as well as to those from whom benefits are expected (Gouldner, 1960). In the Berkowitz-Daniels (1964) experiment, the norm of reciprocity may have contributed to the relatively high help extended by the subject after receiving prior help from the experimenter's confederate. Feelings of obligation arising from prior help may have generalized to the dependent person so that by working hard for her, the subject thought she was reciprocating for the assistance received earlier from the experimenter's confederate.

Goranson and Berkowitz (1966) attempted to separate the effects of the norm of social responsibility from those of reciprocity on helping behavior. They found that when subjects were required to work for the *same* person who had helped them, they worked hardest for that person (i.e., reciprocated most strongly) when his prior assistance had been given them voluntarily, less hard when assistance had been required, and least hard when it had been refused. They concluded that an individual feels obligated to reciprocate prior help when this help is given voluntarily. However, the hypothesized social responsibility norm could also have been operative to some extent.

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In addition to conjectured norms, there are other factors which may produce helping behavior. Individuals often behave in a way that they perceive is expected by others in order to achieve their approval. Kelman (1958) calls this process *compliance,* as opposed to *internalization* where the individual behaves in a certain way because the content of the behavior is intrinsically rewarding to him. According to Crowne and Marlowe (1964), people describe themselves in a socially desirable way in order to obtain approval from others. Extending this line of thought to Goranson and Berkowitz (1966), the subject's help for the same person who helped her previously may have been influenced by considerations of social desirability—the subject may have helped the dependent person in order to win hers or the experimenter's approval by giving the same person the help the subject perceived was expected.

Another possible determinant of helping in an experiment may be seen as the "demand characteristics" which Orne (1962) described as cues in the procedure and instructions which convey the experimenter's true hypothesis. For example, in the Goranson-Berkowitz experiment, although subjects were misled as to the experimenter's hypothesis, the experimental situation may have become relatively transparent to the subject. She might have perceived that helping was the behavior expected by the experimenter since she was required to help the same person who helped her. Assuming that the subject is motivated to be a *good* subject by validating the experimenter's hypothesis (Orne, 1965), her help for the same person who helped her may have been a function of the demand characteristics rather than the independent variables.

The present experiment will examine the relative contributions of the norms of social responsibility and reciprocity while controlling for other possible determinants of helping behavior.[3] In Phase I, the subject was led to believe that she was highly dependent on one person's help, *P1*, for attainment of a prize. In Phase II, the subject was told that a second person, *P2*, was highly dependent on the subject's help for attainment of a different prize. Thus, in both phases of the study the subject, theoretically, was aware of the general social responsibility norm which prescribes that it is appropriate to aid those needing help. By making *P1* different from *P2*, in all experimental conditions, the effects of social desirability are presumably equalized throughout. Since *P1* and *P2* are different persons, the experimental situation should not, theoretically, be transparent to the subject, thereby diminishing the chances that the subject guesses the true hypothesis.

The response elicited by the reciprocity norm, the repayment response, is conceptualized here as operating according to the principle of stimulus generalization. That is, the repayment response is regarded as capable of generalizing from the initial source of helping, *P1*, to other similar persons, *P2*, with greater strength of the helping response the greater the similarity. If the effects of prior help are mediated by the reciprocity norm, there should be a generalization of the repayment response and thereby high help to *P2* only when *P1* and *P2* are similar, with little help given to *P2* when *P1* and *P2* are dissimilar. If, however, prior help heightens the salience of the norm of social responsibility which prescribes that it is appropriate to extend high help to others, then high help should be given to *P2* regardless of *P2*'s similarity to *P1*. If both reciprocity and social responsibility are operative, more help should be given to *P2* when *P2* is similar to *P1* than when *P2* is dissimilar to *P1*. If the subject's help for *P2* (when *P1* and *P2* are similar) is a function of both norms, there is, as yet, no way of assessing the relative contributions of the two norms. In order to separate further the effects of the two norms, the present experiment also investigates the effect of *hindrance* from $P1$ on the subject's subsequent behavior towards $P2$. After prior hindrance, reciprocity should produce hindrance for $P2$ only when $P1$ and $P2$ are similar. Emphasis is placed here not on the repayment of help but on the return of injuries, and is found in the *negative* norms of reciprocity (Gouldner, 1960). Social responsibility should not be a major determinant here since the social responsibility norm prescribes help rather than hindrance. After prior hindrance, the existing norm of social responsibility should produce help for $P2$ when $P1$ and $P2$ are dissimilar. If, however, prior hindrance establishes a new norm of hindrance which prescribes hindrance as appropriate behavior in this situation, then hindrance should be expressed towards $P2$ regardless of $P2$'s similarity to $P1$.

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[3] One of the variables which was controlled was the salience of the social responsibility norm. In order to eliminate its differential effects on helping among experimental conditions, the salience of the social responsibility norm was maximized throughout by means of experimental instructions.

## METHOD

### *Subjects*

The subjects were 72 young women between the ages of 17 and 21 who were enrolled in the introductory course at the University of Toronto. They were required to serve 4 hours as psychological subjects.

### *Phase I*

The subject was individually told the ostensible purpose of the study, which was to find out how well people evaluate how hard someone has worked on a job. Subjects were told that they would have to work for a period of time after which their work would be rated according to two criteria, and that they stood to win points according to each. One criterion, an objective one, was "productivity": 5 points for every article made. Another criterion was "motivation to work" as judged by another person. The experimenter explained that every subject would be required to perform two tasks: She would be asked to work on a job after which another subject would evaluate her motivation to work. Each subject would also be required to evaluate someone else's motivation to work on a different task. Presumably, subjects were called together in groups of three, none of whom would ever know who the others were. Actually, there was only one subject present during each session. Using an (ostensible) randomization procedure, the subject was to be evaluated *first* by another girl, $P1$, then the subject was to assess a different girl, $P2$.

The subject was asked to fill in the first eight items on an attitude and value questionnaire consisting of 11 items. This questionnaire is a modified version of one devised by Kaufmann and Zener (1966). The subject was asked to check the alternative response on a 7-point scale which best expressed her degree of agreement or disagreement with each item. The items consisted of general statements in the areas of politics, morality, and religion. $P1$ would see the subject's questionnaire before she rated her motivation presumably to get some idea of what the subject was like.[4] The subject's job was to make paper boxes for 8 minutes and she was to receive a productivity score consisting of 5 points for every box she made as well as a motivation score from $P1$. The assignment of a motivation score was the pretext for the introduction of the help and hindrance manipulations to be explained below. If, on the basis of her examination of the boxes, $P1$ thought that the subject had high motivation, she could add points. $P1$ could add no points if she thought the subject had average motivation, and she could deduct points for low motivation. The subject's overall score consisted of the algebraic sum of points assigned according to the two criteria.

The experimenter explained that of all the subjects in the study who had made boxes, the one with the highest overall score would win a gift certificate. It was emphasized that the subject was not competing with either $P1$ or $P2$ for the same gift certificate—they were ostensibly in different contests. If the subject were to perceive that she was competing with $P2$, she might be reluctant to help her (Deutsch, 1949). All subjects received a productivity score of 65 since they were asked to stop working after they had completed 13 boxes. The experimenter then took the subject's boxes to $P1$ who allegedly assigned the subject a motivation score.

*Experimental manipulations.* The experiment employed a 2 × 3 factorial design. Similarity between $P1$ and $P2$ was introduced on two levels and nature of prior help was introduced on three levels. Subjects were randomly assigned to each of the six experimental treatment conditions.

Differences in *similarity* between $P1$ and $P2$ were created using the same questionnaire filled in earlier by the subject. The subject received two copies of the same questionnaire partially filled in supposedly by $P1$ and $P2$. In the $P1$-$P2$ similarity condition, $P1$ and $P2$ held similar attitudes and values; in the $P1$-$P2$ dissimilarity condition, they showed opposite views. The results of a pilot study showed that the subject tended to see herself as more similar to $P1$ and $P2$ when she had checked a certain set of alternatives than when $P1$ and $P2$ had checked the opposite set of alternatives. Moreover, when the subject perceived herself as similar to $P1$ and $P2$, she saw them as "liberal." She saw the opposite personality as "conservative." In order to control simultaneously for the effects of *P2*'s personality as well as similarity between the subject and *P1*, and between *P2* and the subject, the personalities that *P1* and *P2* ostensibly had were counterbalanced in the similarity and dissimilarity conditions.

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[4] The real reason for asking the subject to fill in this questionnaire was to make the later similarity manipulation believable. The similarity manipulation involved showing the subject two copies of the same questionnaire which were ostensibly filled in by $P1$ and $P2$. In some preliminary work in which the subject was not asked to fill in this questionnaire, some subjects had become suspicious and asked why the other two girls had filled in the questionnaire while they (subjects) did not.

The subject received the questionnaire presumably to get some idea of what the other two girls were like. She was to fill in the last three items on each questionnaire the way *P1* and *P2* would have responded if they had been allowed to complete them. The experimenter explained that the subject's responses would be studied to determine the accuracy of her impression of *P1* and *P2*. The real reason for asking the subject to fill in the questionnaires was to check on the effectiveness of the similarity (dissimilarity) manipulation.

The *nature of prior help* manipulation involved the number of points that *P1* assigned to the subject for "motivation to work." The experimenter brought back the subject's boxes with *P1*'s numerical assessment of the subject's motivation written on the rating sheet which was sealed in an envelope. *P1* had also written the subject a brief message telling her why she had given her that particular motivation score. The subject was asked not to reveal the score she received in order to make more believable the experimenter's later assurance that she would not be seeing the motivation score that the subject assigned to *P2*. These anonymous evaluations should have minimized the effects of social desirability as well as implicit and situational cues.

For the subjects in the prior help condition, *P1* added 38 points which she stated in her message she felt the subject deserved for high motivation to work. In the no prior help condition, *P1* gave the subject no points and she commented that the subject's motivation was about average. In the prior hindrance condition, *P1* deducted 38 points from the subject's productivity score stating that in her opinion the subject had low motivation. The subject then filled in a brief questionnaire consisting of items which assessed the effectiveness of the help manipulation as well as the subject's liking for *P1*. For most items, the subject's responses were scored on a 7-point scale.

### *Phase II*

The subject's task was to evaluate *P2*'s motivation to work on the basis of her examination of nine paper cups ostensibly made by *P2*. The subject was instructed to write *P2* a brief message telling her why she gave her the numerical score. *P2* was supposedly competing with subjects who had made paper cups for a different gift certificate from the one for which the subject was eligible. Both the subject and *P2* had approximately the same productivity score.

TABLE 1

MEAN AMOUNT OF HELP (NUMBER OF POINTS) GIVEN TO *P2* IN EACH OF THE HELP × SIMILARITY CONDITIONS

| Similarity | Prior hindrance | No prior help | Prior help |
|---|---|---|---|
| *P1* similar to *P2* | −12.3 | 6.2 | 34.7 |
| *P1* dissimilar to *P2* | 18.8 | 7.4 | 30.4 |

Note.—$n = 12$ per cell.

TABLE 2

ANALYSIS OF VARIANCE OF THE AMOUNT OF HELP (NUMBER OF POINTS) GIVEN TO *P2* FOR MOTIVATION TO WORK

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Nature of prior help (N) | 2 | 6,127.1 | 111.6** |
| Similarity between *P1* and *P2* (S) | 1 | 1,577.4 | 28.7** |
| Personality of *P2* (P) | 1 | .2 | <1 |
| N × S | 2 | 2,183.7 | 39.8** |
| N × P | 2 | 1.6 | <1 |
| S × P | 1 | .2 | <1 |
| N × S × P | 2 | 2.2 | <1 |
| Error | 60 | 54.9 | |

** $p < .01$.

*Main dependent variable.* The measure of the amount of help or hindrance extended was the number of points for motivation that the subject either added to or deducted from *P2*'s productivity score.

*Checks on the similarity manipulation.* The second check on the similarity manipulation ascertained how well the subject could recall *P1*-*P2* similarity approximately ½ hour after the original manipulation. The subject was to fill in three blank items the way she recalled that *P1* and *P2* had done so on the original questionnaire. Also, she was to indicate the extent to which she agreed with the statement that *P1* and *P2* would have a great deal in common if they knew each other. This question was designed to find out the degree to which the subject's perceptions of similarity in attitude and values between *P1* and *P2* had generalized to their total personality.

The subject then filled in a brief questionnaire consisting of items which assessed the degree to which she thought she had helped or hindered *P2*, as well as her liking for *P2*. In the postexperimental inquiry, the subject was asked (*a*) a series of questions designed to check on the effectiveness of the experimental instructions and (*b*) a series of questions designed to ascertain if the subject perceived the true purpose of the experiment. In the debriefing session the experimenter told the subject that she had misled her and the true purpose of the experiment was explained.

## RESULTS

### *Amount of Help (Number of Points) Given to* P2

Table 1 presents the mean number of points given to *P2* in each of the Help × Similarity conditions. The analysis of variance results are summarized in Table 2. As predicted earlier, there was a significant effect due to the helping variable ($p < .01$): the subject gave $P2$ more points after prior help than after no prior help was received. The similarity variable did not significantly affect the number of points given in either the prior help ($t = 1.82$, $df = 22$, $ns$)[5] or the no prior help conditions ($t < 1$, $df = 22$, $ns$). There was a significant interaction between the helping and similarity variables ($p < .01$), which was due solely to the difference between the similarity and dissimilarity conditions after prior hindrance was received ($t = 9.97$, $df = 22$, $p < .01$): after prior hindrance, the subject gave $P2$ points when $P1$ and $P2$ were dissimilar, and she deducted points when $P1$ and $P2$ were similar. In the dissimilarity condition, there were significant differences in the number of points $P2$ received as a function of the helping variable: $P2$ received more points in the prior help than in the prior hindrance condition ($t = 4.50$, $df = 22$, $p < .01$), and she received more points in the prior hindrance condition than in the no prior help condition ($t = 4.19$, $df = 22$, $p < .01$).

### *Tests of the Effectiveness of the Experimental Manipulations*

*Similarity manipulation.* The first check on the similarity (dissimilarity) manipulation was the degree of similarity between the preferences the subject checked for $P1$ and $P2$ on the last three items of the attitude and value questionnaire. The results of Tukey's (1959) test showed that the distance scores over the last three items were significantly smaller in the similarity than in the dissimilarity condition (total $> 13$, $p < .001$). (The smaller the distance score, the higher the perceived similarity between $P1$ and $P2$.) Further evidence for the success of the similarity manipulation was found in the differences in degree of similarity between the girls in their responses, as the subject remembered them, to three of the questions that had been filled out earlier ostensibly by $P1$ and $P2$. Distance scores were significantly smaller in the similarity than in the dissimilarity condition (total $> 13$, $p < .001$). Thirdly, while subjects in the similarity condition agreed with the statement that $P1$ and $P2$ would have a great deal in common, subjects in the dissimilarity condition disagreed.

*Help manipulation.* Questionnaire I, administered at the end of Phase I, included items to test the effectiveness of the help manipulation, a measure of the subject's liking for $P1$, and a question which asked the subject the criteria $P1$ used in assessing the subject's motivation. One of the items asked the subject the extent to which $P1$ had helped her win the gift certificate. An analysis of variance of these data showed a significant main effect for the helping variable ($F = 339.5$, $df = 2/66$, $p < .01$) and a significant linear trend for the three "help" conditions ($F = 677.1$, $df = 1/66$, $p < .01$). In the prior help condition, the subject tended to state that she had been helped, while in the no prior help condition the subject stated that she was "neither helped nor hindered." In the prior hindrance condition, the subject tended to state that she had been hindered.

Other questionnaire results point to the differential degree of satisfaction engendered by the "help" manipulation. There was a significant effect due to the help manipulation ($F = 68.3$, $df = 2/66$, $p < .01$), and a significant linear trend for the three "help" conditions ($F = 135.5$, $df = 1/66$, $p < .01$). A subject who received prior help from $P1$ indicated greater satisfaction with her rating than a subject who received no prior help, while a subject who was hindered was relatively dissatisfied with $P1$'s rating.

The helping variable had a significant effect on the subject's liking for $P1$ ($F = 10.1$, $df = 2/66$, $p < .01$). $P1$ was liked significantly more in the prior help condition than in the no prior help and prior hindrance conditions combined.

Subjects in each of the six experimental conditions stated that they thought $P1$ had used the following five types of criteria in assessing their motivation to work: quantity and/or quality, difficulty of the task, the subject's attitude and value questionnaire, criticisms of the boxes, and other criteria which include intuition, etc. There appeared to be no difference in the frequency with which each of the different criteria was used among conditions.

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[5] **All tests of significance reported are two-tailed.**

### *Test of the Subject's Awareness of Amount of Help or Hindrance Extended to* P2

Questionnaire II, administered at the end of Phase II, included items which assessed the degree to which the subject thought she had helped or hindered *P2*, a measure of the subject's liking for *P2*, and a question which asked the subject which criteria she used in assessing *P2*'s motivation. One of these items asked the subject to what extent she had helped *P2*. There was a significant difference between the responses of subjects in the prior hindrance condition (when *P1* and *P2* were similar) and the responses of subjects in the other five treatment conditions. When *P1* and *P2* were similar, and the subject deducted points, the subject stated that she had hindered *P2*; subjects in the other conditions who gave points tended to state that they had helped *P2* to some extent. The subject's liking for *P2* was not related to the differential help treatments ($F < 1$, $df = 2/66$, *ns*): Subjects who hindered *P2* did not like her less than did subjects who helped *P2* (see Table 3).

Subjects in each of the six experimental conditions stated that they had used the following five types of criteria in assessing *P2*'s motivation to work: quantity and/or quality, difficulty of the task, *P2*'s attitude and value questionnaire, criticisms of the cups, and other criteria such as intuition, etc. There appeared to be no difference in the frequency with which each of the different criteria was used among conditions.

### *Analysis of the Subject's Message to* P2

The subject's message was analyzed by computing the frequency with which a number of criteria were mentioned, such as quantity and/or quality of the cups, difficulty with the task, faults with the cups, etc. Although subjects in all conditions found fault with the cups, subjects in the prior hindrance condition (when *P1* and *P2* were similar) mentioned faults most frequently and used them as their sole basis for their minus ratings of *P2*'s motivation. It appears that subjects who deducted points were able to substantiate their minus ratings by referring to the faults with the cups. While subjects in the other conditions noticed the faults, generally, they made it clear to *P2* that they were not penalizing her for them, and in some cases these faults were taken as further evidence of "high motivation" since they were signs of *P2*'s haste. There appeared to be no difference in the frequency with which the other criteria were used among the conditions.

TABLE 3

MEAN SCORES: ITEM 4, QUESTIONNAIRE II: "IF YOU MET NUMBER 23, HOW MUCH DO YOU THINK YOU WOULD LIKE HER?"

| Similarity | Prior hindrance | No prior help | Prior help |
|---|---|---|---|
| *P1* similar to *P2* | 5.2[a] | 5.3 | 5.6 |
| *P1* dissimilar to *P2* | 5.5 | 5.3 | 4.8 |

Note.—Number 23 refers to *P2*.
[a] High scores indicate high liking.

### *Results of the Postexperimental Inquiry*

1. The subject was asked questions concerning some of the deceptions. All subjects stated that their work period was 8 minutes. When asked when the experimenter would see the motivation score they gave to *P2*, all subjects replied that the experimenter would not see it. They stated that they would never meet or know who the other two girls were.

2. When asked about the purpose of the experiment, subjects repeated, for the most part, what the experimenter had told them in the instructions. None of the subjects guessed the true purpose of the experiment or the questionnaires.

## DISCUSSION

The results supported the hypothesis that prior help increases the salience of the norm of social responsibility which prescribes that it is appropriate, in this situation, to extend high help to dependent others—in the prior help condition, the subject was extending high help to *P2* regardless of her similarity or dissimilarity to *P1*. Further, evidence for the operation of the social responsibility norm in the no prior help condition is found in subjects' responses to the question of how much they thought they had helped (or hindered) *P2*. Most subjects stated that they had helped *P2* to some extent.

There are two, perhaps related, alternative explanations for the subject's behavior toward *P2* after prior hindrance. The subject's hindrance of a similar *P2* may have been a function of the negative norm of reciprocity (or the talion law of "an eye for an eye") which prescribes that it is appropriate to return an injury after receiving hindrance. *P2*'s similarity to *P1* presumably functioned as a cue which activated the negative norm of reciprocity and thereby resulted in the subject's reciprocating *P1*'s hindrance by hindering *P2*. Since all subjects stated that they had hindered *P2*, there is evidence that subjects were *not* behaving according to the general prescription that one does not hinder a dependent person. Because all subjects in the prior hindrance condition deducted fewer points from *P2* than *P1* had deducted, it may be that the subject was aware of the social responsibility norm which prescribes help.

Another explanation for the subject's behavior in the prior hindrance condition is based on a model proposed by Berkowitz (1964), which states that prior frustration induces a tendency to aggress, and the strength of the aggressive response is a direct function of the degree of association or similarity between the object of the potential aggression and the anger instigator. Presumably, *P2*'s similarity to *P1* influenced the strength of *P2*'s "aggressive cue" value which elicited hindrance towards *P2*. In other words, hostility was displaced from *P1* to *P2* according to the principle of stimulus generalization. The subject's resentment towards *P1* was expressed in her responses to Questionnaire I, that she was dissatisfied with *P1*'s assessment of her, and in her responses in the postexperimental inquiry, that she was annoyed by *P1*'s "capricious" behavior. The subject, however, did not extend the total amount of hostility that she might have because of the normative requirements of the situation which prescribed help to *P2*. Thus, when *P1* and *P2* were dissimilar, neither aggressive cues nor conditions necessary for the operation of the reciprocity norm were present, and the social responsibility norm became a major determinant of the subject's relatively high help for *P2*.

The subject's behavior in this study was probably not a function of the demand characteristics since the results of a "nonexperiment"[6] suggested that the true purpose of the study was not readily apparent to the subject.

An alternative explanation for the results is that the differential help conditions produced a judgmental effect. That is, subjects in the three help conditions learned to employ different criteria in judging *P2* on the basis of the criteria they perceived were employed by *P1*. However, there were no differences among conditions in the criteria subjects used to judge *P2*.

Conceivably, subjects in the prior help condition might have been put in a good mood as a result of their gain and their good will generalized to *P2*. On the other side, there was evidence that subjects in the prior hindrance condition were resentful. If the subject's hindrance of *P2* was due to a generalization of resentment from *P1* to *P2*, there should also have been a generalization of dislike from *P1* to *P2*. There was no evidence, however, that *P2* was liked any less when she was hindered than when she was helped. These findings are consistent with the reciprocity hypothesis whose origins are perceived in social norms while asserting nothing about affective responses.

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[6] The nonexperiment is used to ascertain whether or not the true experimental hypotheses are conveyed to the subject by the demand characteristics (Riecken, 1962). The procedure involved giving the subject the instructions that were destined for an actual experimental subject and asking her to imagine how she would behave *if* she were a subject in the experiment. The experimenter then asked the subject a series of questions concerning what she thought was the true purpose of the experiment (Orne, 1959).

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# SOME EFFECTS OF GUILT ON COMPLIANCE[1]

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Two experiments studied conditions under which compliance will be increased. Experiment I showed that guilt (induced by having subjects deliver painful electric shocks to a confederate) will sharply increase compliance. Status of the confederate and presence of a witness had no effect. Expriment II differentiated among possible explanations for this effect, and suggested that guilt, rather than sympathy or a desire to make restitution, was the critical variable. Subjects who had observed but not delivered the shocks did not comply. Compliance was highest when the request was made by someone who had observed the subject deliver the shocks, but was not himself the victim.

Most research concerned with how to get a person to comply has been carried out in situations where there is a great deal of external pressure for compliance. In many studies, the extent of the pressures is obvious (e.g., Asch, 1951; Deutsch & Gerard, 1955). Other studies have used the immense pressure of the experimenter-subject relationship. For example, Milgram's (1964) remarkable demonstrations of obedience are all carried out in the context of an experimenter ordering a subject to comply. In these, and in most other studies of compliance, the focus has been on variables affecting compliance within this framework of strong external pressure.

The research described here studies how one can get compliance without these external pressures. This is of interest for several reasons. First of all, it is often necessary, for financial, ethical, practical, or other reasons, to apply only minimal external pressure when attempting to obtain compliance. Second, it has become clear from much of the research stemming from dissonance theory (e.g., Brehm & Cohen, 1962; Carlsmith, Collins, & Helmreich, 1966; Festinger & Carlsmith, 1959) that in order to get maximal attitude change following compliance, it is important to have as little pressure for compliance as is consistent with obtaining that compliance. All of this work has ignored the question of how to get compliance with minimal pressure. In all cases the compliance was obtained within the experimenter-subject relationship, with an experimenter requesting that the subject comply. As Orne (1962) pointed out, compliance is virtually guaranteed under these circumstances. In the dissonance studies, variations in pressure for compliance are obtained by adding a number of additional external pressures (e.g., money) for some of the subjects.

More recently, there has been some interest in the question of obtaining compliance with minimal pressure. Wallace and Sadalla (1966) attempted to persuade subjects to volunteer for an experiment involving painful shocks. Some of their subjects were induced to break an expensive machine before the request was made; for other subjects the machine was broken but the subject was not responsible. Subjects who had broken the machine were significantly more likely to volunteer for the unpleasant experiment than subjects who had not. Unfortunately, 30% of the subjects never broke the machine, so that such a comparison is confounded by self-selection.

A similar finding is reported incidentally by Brock and Becker (1966). Again, student subjects were induced to press a button which either destroyed the experimenter's apparatus or emitted a slight puff of smoke. Following this, the experimenter asked them to sign a petition advocating doubling tuition at the University. No subjects in the low-damage condition were willing to sign the petition. About 50% of the subjects in the high-damage condition were willing to sign it.

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Both of these experiments suggest that guilt may cause a sharp increase in compliance. Unfortunately, in both experiments the request was made by the experimenter who was fully aware of the subject's condition. We may expect compliance to be strongly affected by the persuasiveness or perseverance of an experimenter's request, as well as by more subtle variations in his behavior. Accordingly, the possibility of some experimenter bias creeping in is disquieting. Nevertheless, the experiments do strongly suggest that guilt may play an important role in inducing compliance.

In the first experiment reported here, the authors tried to assess the effects of three different variables which might be thought to produce compliance. In all cases, the request for compliance was made by someone who was not an experimenter, and the request was not made as part of an experiment.

One variable of some interest in the compliance paradigm is the status of the person making the request for compliance. Accordingly, some of the subjects in the present study were asked to comply by someone of higher status than themselves; others were asked to comply by someone of lower status. A second variable, which might be expected to interact with status, is the presence or absence of a witness to the request. Finally, the authors wished to study the effects of guilt, and thus induced some of the subjects to deliver painful electric shocks to the person making the request.

To summarize, the first experiment employed a 2 × 2 × 2 factorial design to test the effects of three variables on the likelihood that a subject will comply to a request. The requester had either been shocked or not shocked (by the subject) prior to the request; he was introduced as either a high- or low-status person; and the request was made either privately or witnessed by an experimenter.

## EXPERIMENT I: METHOD

### *Overview of Design*

The dependent measure was the extent to which the subject was willing to comply to a request by a confederate who had just served as learner in a bogus experiment. Subjects were assigned the role of "teacher" in a learning situation, and were instructed to throw a large knife switch whenever the learner made a mistake. The switch merely sounded a buzzer, but half of the subjects had been told that closing the switch also caused the learner to receive painful electric shocks. After the experiment was completed, the learner, who had been described as being drawn from either a high- or low-status pool of subjects, presented a request to the subject either privately or in the presence of the experimenter.

### *Subjects*

Male students of Foothill College were offered $2 to participate in a 45-minute learning experiment to be conducted at the Stanford University Psychology Department. Forty-eight students were recruited, and data from 40 subjects, 5 randomly assigned to each of eight conditions of the 2 × 2 × 2 design, were used. Data from 8 subjects were eliminated from the analysis, 7 because they suspected that the confederate or his request were part of the experimental design, and 1 because he did not believe that the confederate was actually being shocked. None of the results reported below is altered if these subjects are included in the analysis.

### *Apparatus*

The experimental room contained three tables and an impressive array of wiring and electrical equipment. Two booths, one for the learner-confederate, and one for the teacher-subject, faced each other on the center table. The learner's booth was equipped with response switches which turned on corresponding lights in the teacher's booth, a panel of feedback lights which were activated by switches on the teacher's side, and a book containing 15 patterns, described as a standard concept-formation task.

In addition, a respirometer tube which was connected to a pen recorder, and palm electrodes with leads running to a Fels dermohmmeter, were present at the learner's side. In shock conditions the palm contacts were described to the subject as electrodes hooked up to a shock generator; in nonshock conditions they were called palm recorders connected to a skin-resistance measuring device. The purpose of this equipment was simply to make it credible to the subject that the confederate's responses were the main focus of the experiment.

Aside from the complementary switches and panels necessary for viewing the learner's responses and feeding him back "correct answers," the subject's booth contained controls labeled "correct" and "error." The subject was instructed to use these switches to reinforce the learner positively when he was correct, and to give him negative feedback after trials which included at least one error. Pushing the positive button turned on a large green light in both booths, and closing the negative knife switch sounded a loud buzzer which operated for 1 second at 1-second intervals. The third table, which was empty except for pencils and a letter basket, was used for making the request after the learning trials.

### *Procedure*

When subjects arrived they were informed that they would be working with another man who would arrive in a few minutes. Meanwhile the experimenter explained the rationale for assigning the subject to the role of teacher:

> What we're trying to find out is what kinds of things affect the way people solve problems which require them to learn fairly difficult concepts. One thing we already know for sure is that certain kinds of feedback information can improve a subject's performances. But, we think *who* gives the feedback information also makes a big difference; that is, we think learners may respond in different ways to different teachers. As you probably know, the teacher or trainer who gives the feedback in these kinds of studies is almost always an experimental psychologist or at least an advanced graduate student, but in this series that we're running now we want to find out how our subjects react and learn when they *know* that a freshman or sophomore junior college student like yourself is acting as the instructor—the one who gives the feedback.

At this point the experimenter remarked that the experiment would be particularly interesting because a special group of men had been recruited to act as learners. The experimenter then manipulated the status of the requester by identifying the group as (*a*) "young executives from Lockheed who have been selected for a special management development program" or (*b*) "unemployed men who were high school dropouts and are now being processed for eligibility in the Federal Job Training Program." The experimenter then explained exactly what the subject was to do during the learning trials:

> He [the learner] is going to work on this standard concept-formation task. Your job will be to feed back the correct answer on each of the 15 trials like this [experimenter demonstrates switches]. You record his answers here [subject is handed a form which includes a list of correct responses and columns for recording the learner's responses. The learner's source group—"federal unemployment" or "management development"—and the learning method—"shock" or "negative feedback"—were prominently written in at the top of the page], and then feed back the correct configuration by using these switches which activate his lights. If he is completely correct you give him about 2 seconds of positive feedback like this [experimenter demonstrates switch which operates green light], but if he is wrong—that is, if he made any mistakes on the trial—you throw this negative feedback switch which sounds a loud buzzer. . . .

In the shock conditions the experimenter continued:

> and activates that shock apparatus [points at dermohmmeter] which will administer shocks to Mr. Rawlins' hands. I'll show you how it works when we look at his side of the apparatus. We regulate these shocks so that they are pretty painful and unpleasant—otherwise we can't really see how they affect the learning process—but there is absolutely no permanent tissue damage resulting from these shocks, so don't worry about that. [Experimenter and subject walk around table to learner's booth.] . . . We also attach one of these electrodes to each of the subject's palms. As you can see, these are hooked to the generator in order to apply the shock voltage at a level which is as painful as we can get commensurate with the subject's safety. He's already had a dose of the shock last week, so he knows what it feels like.

In nonshock conditions, the experimenter explained that a buzzer signal given after wrong responses sometimes has a facilitating effect on learning, and when the subject was shown the electrodes on the learner's side of the table, he was told "they will be attached to each of the subject's palms . . . it measures skin resistance much in the same way as they do with lie detectors."

After the subject was reseated at his booth, questions about the procedure were answered, after which the experimenter opened a door leading to a room where the confederate was waiting, greeted him, and introduced him to the subject.

The two booths were arranged so that no visual contact could be made between learner and teacher while they were seated. The experimenter then briefly recapitulated the procedure. In both shock and nonshock treatments the buzzer was ambiguously referred to as negative feedback in the presence of the confederate—for example,

> Just like last time, when the answers don't match —when you're wrong—the teacher throws his negative feedback switch. Just a second, let me check this [the experimenter glances at some dials]. OK, let's see if the switch is working. [to teacher] Remember, just give three bursts of negative feedback.

After the buzzer sounded three times, the experimenter said humorously to the learner, "Well, I guess that's working all right."

The confederate's responses were programmed so that he appeared to be learning the concept toward the end of the series. Nevertheless, he was completely correct only six times during the 15 trials, so that all subjects were required to throw the negative feedback switch nine times. Four of the nine times that he made an error, the confederate kicked the table in response to the buzzer. This lent realism to the shock conditions; subjects in nonshock conditions could attribute the slight movement to a change of position, if they noticed it at all.

### *Request*

After the fifteenth trial the experimenter removed the confederate's palm contacts, and asked him to sit at another table to fill out a "concept-formation questionnaire." As soon as the confederate began writing, the experimenter seated the subject at the same table, paid him, and asked him to complete a receipt which called for address, telephone number, and other personal information. The receipt form was designed to occupy the subject for 1 or 2 minutes. Both the confederate and the subject were told that the experiment was over, and that they were free to leave when they finished filling out the forms which they were to put in a letter basket on the table.

In witness conditions, the experimenter began adjusting the apparatus while hovering near the table and obviously attending to the request. Near the beginning of the request, the confederate turned to the experimenter and asked, "Am I bothering you?" to which the experimenter replied, "No, I was just interestd in what you were saying." In no-witness conditions, the experimenter excused himself and left the room, closing the door after him. A concealed microphone allowed him to listen to the request from another nearby room.

A few seconds after the experimenter either left the room or began adjusting equipment, the confederate completed his questionnaire, put it in the letter basket, and haltingly began his rehearsed request. After engaging the subject's attention, the confederate identified himself as a member of a committee that was attempting to prevent a freeway from being built through the redwood trees in Northern California.[3] He explained that people were needed to telephone potential signers of a petition to save the trees. After displaying several stacks, each made up of 50 index cards containing names and phone numbers, he requested that the subject help with this task:

> What's involved is just to phone the people and say there is this petition and would they be interested in signing it. So, what I was wondering was if you'd be willing to call some of these people, but you wouldn't have to call up 50 of them if you didn't have time. As I say, it takes a minute or two for each one. Would you be willing to call some of these people for us?

If the subject refused to help, the confederate put the cards in his pocket and left the room. If the subject agreed to make some phone calls, he was asked, "How many of them do you think you could do? As I say, we've got 50 of them, but any number you could do would be a help."

Shortly after the subject refused, or complied and specified the number of calls, the experimenter (who then reentered the room in nonwitness conditions) interrupted, and asked the confederate to wait in another room because he had "forgotten to ask the subject some questions." After the experimenter determined whether or not it was suspected that the request had been part of the design, he explained the actual purpose of the experiment to the subject, and invited him to return at a later date to pick up a copy of the results.

This procedure effectively prevented the confederate from knowing whether the subject was in the shock or nonshock condition. He was also ignorant as to whether he (the confederate) was of high or low status. It was, of course, impossible for him to be ignorant as to whether or not there was a witness present. The fact that the person making the request was blind (on at least two of the three variables) seems to us a critically necessary feature for studying compliance. As pointed out by Aronson and Carlsmith (1968), bias cannot affect interactions if the experimenter is blind on one of the interacting variables.

TABLE 1

Mean Number of Phone Calls Accepted

| Condition | Shock | | Nonshock | |
|---|---|---|---|---|
| | Requester's status | | Requester's status | |
| | High | Low | High | Low |
| *E* present | 26 | 16 | 6 | 2 |
| *E* not present | 25 | 29 | 5 | 3 |

## Results and Discussion

The results can be summarized simply. Neither the status of the person making the request nor the presence of a witness had any effect on compliance. Both of these variables also failed to interact with each other or with the presence or absence of shock. When the subject had shocked the confederate prior to the request for compliance, however, there was a striking increase in the amount of compliance elicited. Whereas 25% of the subjects agreed to make some phone calls in the control conditions, 75% of the subjects who had just administered a shock to the confederate agreed to make some calls. A Fisher exact test on this difference yields a probability of .004. Table 1 shows the mean number of phone calls accepted in each of the eight conditions. Table 2 shows an analysis of variance of the data. Again, we see that the effect of delivering a shock to the person who makes the request for compliance is highly significant; no other effects even approach significance.

Not only was the probability of compliance higher in the shock conditions, there was also

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[3] In view of recent publicity and concern about redwoods in California, it seems worthwhile to mention that the first experiment was carried out in March, 1965; the second experiment was carried out in August, 1965. At that time, there was relatively little concern about redwoods; none of the subjects had heard anything more than vague talk about conservation groups.

TABLE 2

ANALYSIS OF VARIANCE

| Source | *df* | *MS* |
|---|---|---|
| Shock (A) | 1 | 4,000 |
| Status (B) | 1 | 90 |
| Witness (C) | 1 | 90 |
| A × B | 1 | 0 |
| A × C | 1 | 90 |
| B × C | 1 | 160 |
| A × B × C | 1 | 90 |
| Within cell error | 32 | 279.38 |
| Total | 39 | |

a tendency for the *level* of compliance to be higher. Among those subjects who did comply, the mean number of phone calls agreed to was 32 in the shock conditions, and 16 in the nonshock conditions ($t = 1.93$, $p < .10$). Of the 20 compliant subjects, 7 agreed to take the whole stack of 50 cards; all of these were in the shock conditions.

Although this experiment provides convincing evidence that delivering a shock to another person will markedly increase the likelihood of subsequent compliance to that person, the interpretation is somewhat ambiguous. At least three alternative explanations come immediately to mind. First, it may be that people will in general comply with a request from a person whom they see as unfortunate. Since the confederate has just been subjected to a series of painful shocks, it may be that the subjects feel sorry for him, sympathetic towards him, and comply with his request in order to make his life a little more pleasant.

Alternatively, it may be that the subjects feel very guilty about having administered a series of painful shocks to another person, and will engage in compliant behavior for anyone who asks them to help in a good cause. This explanation suggests that the person feels guilty, or that his self-image has been tarnished, and that compliance with another may be a good way to alleviate the guilt or to gild the image. A third possible interpretation, related to the last, is that the subject wants to make restitution. Having injured the confederate by delivering painful shocks to him, he wishes to compensate for this by doing a favor for him. For convenience these three interpretations will be referred to as the sympathy, the generalized guilt, and the restitution explanations.

Since each of these is perfectly plausible in the original study, a second experiment was carried out designed to differentiate among them and to provide a sharper theoretical picture of the forces involved. The second experiment used a procedure quite similar to the first, but added a third person to the interaction—a witness. Several of the possible permutations of one person asking another to comply allow one to distinguish among the three possible interpretations offered.

## EXPERIMENT II: METHOD

### *Overview*

The setting for this experiment was essentially the same as for the first. There were, however, three people in the experimental room (two of whom were confederates). Just as before, one person was the teacher for a simple concept-learning experiment, and this teacher either did or did not deliver shocks to a learner. A third person, the witness, served as an observer and his ostensible task was to record all of the responses and feedback. After the learning phase of the experiment was completed, one person was excused, and the subject and one confederate were left alone for a moment to fill out some forms. After a moment, the confederate described the campaign to save the redwoods and asked the subject to make some phone calls to help him.

There were four experimental conditions. Two were an exact replication of the previous experiment. In the control condition, the learner made the compliance request of the teacher, and no mention was made of electric shock. In the restitution condition, the learner made the request of the teacher, and the teacher had just delivered nine electric shocks. In the generalized guilt condition, the witness made the request of the teacher, after the teacher had delivered shocks to the learner. Finally, in the sympathy condition, the learner, who had just received the shocks, made the request of the witness, who had observed, but not delivered, the shocks. Since the true subject was always the person of whom the request was made, he was sometimes in the role of teacher and sometimes of witness. The confederates took the other two roles.

Various comparisons among the conditions make it possible to distinguish among the three possible explanations listed above. Thus, if the generalized guilt condition is significantly higher than the control condition, we can rule out the restitution explanation. Comparing the sympathy condition with the restitution condition enables us to find out whether or not sympathy is a possible explanation.

Again, we felt that it was crucial that the person making the request be blind as to the condition the subject was in. This presents some difficulties, since it also implies that he did not even know what role he was playing. However, the confederate was effectively blinded so that in no case did he know what condition the subject was in.

### *Subjects*

Forty-seven male Foothill College students were paid $2 to participate in the study which was disguised as a learning experiment. Data for 7 subjects were discarded; 3 did not believe that the learner was shocked, 2 could not correctly identify the role of the requester, 1 suspected that the request was part of the experiment, and 1 would not respond to the request without first consulting his parents. The remaining 40 subjects were divided equally among four experimental conditions.

The experimental setting was the same as in the first experiment, except that the table was partitioned into three parts, one for each of the participants. The participants could not see each other when seated. The subject and one confederate arrived for the experiment first. This confederate was always the one who made the request and will be referred to as the "requesting confederate." He and the subject were introduced and spent a minute or two together so that this confederate would be salient for the subject. Then the subject was taken upstairs to the experimental room and the confederate was asked to wait outside "since you were in the experiment last week and know how it works." The experiment was explained to the subject, and his role was made clear. Special efforts were made to also make clear what the role of the first confederate would be. After the experiment had been explained to the subject, both confederates were brought in. The "requesting confederate" always sat at the same place and actually played the same role, although the subject was told that the confederate played whatever role the experimental condition called for. Before the "requesting confederate" was brought into the room, the apparatus was moved (without knowledge of the subject) so that it appeared the same to the "requesting confederate" in all conditions. Thus the "requesting confederate" was blind as to the experimental condition, but in the prior briefing of the subject a great deal of stress was placed on who played what role so that the subject would know who was making the request at the end of the experiment.

After all participants were seated the instructions were briefly reviewed. The experimenter pointed to each spot as he reviewed the instructions so that again the subject was reminded of which confederate was playing the role. The "requesting confederate" could not see the experimenter during this period. The learning phase of the experiment was identical to the previous experiment, except that the witness recorded everything which took place. After this was completed, the experimenter announced that the experiment was over, excused one confederate, and asked the subject and the "requesting confederate" to fill out some receipt forms. He then left the room. While the subject and the confederate were filling out the forms, the confederate made his request for compliance, just as in the first study.

TABLE 3

| Experimental condition | Requesting confederate's role | *S*'s role | Feedback | *M* No. phone calls accepted |
|---|---|---|---|---|
| Control | Learner | Teacher | Buzzer | 13.0 |
| Sympathy | Learner | Witness | Shock | 6.5 |
| Restitution | Learner | Teacher | Shock | 23.5 |
| Generalized guilt | Witness | Teacher | Shock | 39.0 |

Following this, the experimenter returned, asked the subject for any suspicions he might have had, and ascertained that the subject knew what role the "requesting confederate" had played. He then explained the true purpose of the experiment.

## RESULTS AND DISCUSSION

The results of the experiment are shown in Table 3, which lists the average number of phone calls the subjects agreed to make in each of the four conditions. A one-way analysis of variance shows highly significant treatment effects ($F = 7.03$, $df = 3/36$, $p < .001$). It may be seen that the result of the first experiment replicates nicely. Restitution subjects accept an average of 23.5 phone calls; controls accept only 13 ($t = 1.39$, $df = 36$, *ns*). The smaller number of subjects in these two cells prevented the difference from being significant, although the difference is of roughly the same order of magnitude as in the first experiment. Much of the difference in the absolute values of the two control groups is accounted for by one aberrant subject in the control condition in the second experiment who agreed to make 50 calls.

The results are even clearer when we consider the other conditions, and ask whether guilt, restitution, or sympathy is playing the predominant role. Subjects in the generalized guilt condition, where a witness, who has observed the whole interaction, makes the request, agree to make an average of 39 calls. This group is significantly higher than any of the other three conditions ($t \geq 2.05$, $p \leq .05$).

The sympathy condition, on the other hand, produces less compliance than any other condition. In this case, the subject is present when a confederate ostensibly receives the painful electric shocks, but has nothing to do with delivering them. When this confederate later asks the subject to comply, the average number of phone calls accepted is only 6.5, a value which is significantly less than either of the conditions in which the subject actually delivers the shock. Thus a person who has just received a series of painful shocks can elicit compliance only from the person who has administered the shocks, not from a witness to the experience.

Experiment II was designed to distinguish between guilt, restitution, and sympathy interpretations of the large amount of compliance produced by shocking another person. The answer seems clear. Sympathy in itself elicits surprisingly little compliance. In fact, those nonguilty subjects who witnessed a victim receiving shocks were slightly less willing to accede to the victim's request than were control subjects. It appears that guilt arising from personal implication is a necessary precondition for obtaining compliance in this situation. Subjects who were personally responsible for the shocks (restitution and generalized guilt conditions) agreed on the average to phone 31.25 people, whereas subjects who did not administer shocks (sympathy and control conditions) agreed to only 9.75 phone calls ($t = 3.97$, $p < .001$).

Although Experiment I demonstrated that people will attempt to alleviate guilt by complying with the person they feel guilty about, the strong compliance obtained in the generalized guilt condition makes it clear that guilt can lead to compliance even when there is no opportunity to make amends to the injured party. Restitution does not appear to be a necessary component of the guilt-compliance relationship. At first it appears that the witness' knowledge that the subject had hurt the confederate might be critical in explaining these results—that the subject wants to show the witness that he really is a kind and helpful person in order to repair his social image. However, a recent series of experiments by Freedman, Wallington, and Bless (1967) demonstrates compliance effects in similar situations when the request was made by someone who had no knowledge of the subject's transgression.

It is surprising that the generalized guilt condition produces *more* compliance than the restitution condition. Although the comparison between these two conditions is slightly confounded (the witness had not received shock the week before whereas the learner had), the results strongly suggest that there is some other factor which is reducing the tendency to comply in the restitution condition. This unexpected increase in compliance to the witness may reflect a tendency to avoid the victim. Continued contact with the victim could result in uncomfortable feelings of obligation or serve as an unpleasant reminder to the subject that he had voluntarily pushed the shock button. Freedman et al. (1967) provide some evidence to support the avoidance interpretation. The experimenters contrived to induce the subject to knock over a stack of supposedly carefully indexed note cards. Later, when subjects were asked to assist in a public opinion survey, subjects who did not have to associate with the graduate student who owned the cards complied more than nonguilty controls. There were no differences between experimental and control subjects when the request involved working closely with the graduate student victims. A related interpretation is that voiced by several of the present subjects—that the confederate was taking advantage of a situation which had given him some power. As one subject said, "I didn't like that guy. He knew he had me in a situation where I owed him something and he took advantage of it by asking me to make all those phone calls for him." This is also consistent with Lerner's (1965) finding that if a person has inflicted suffering on someone, he is likely to devalue the victim.

Taken together with other recent work (e.g., Freedman et al, 1967), these two experiments provide convincing evidence that a very powerful technique for obtaining compliance is to first induce a person to do something which harms another person. The second experiment reported here suggests that guilt about the action is the mediating factor in producing this compliance, and that the compliance need not be to the person who was hurt. The wide variety of methods of hurting another person which have been used in experiments reporting this relationship, including destroying a machine (Brock & Becker, 1966; Wallace & Sadalla, 1966), costing another person green stamps (Berscheid & Walster, 1967), upsetting the order of a graduate student's index cards (Freedman et al., 1967), and lying (Freedman et al., 1967), give the result substantial empirical generality.

It is not clear, however, why compliance to the types of requests that are typically made in these experiments is so effective in alleviating the guilt. The fact that the compliance need not be to the person who is responsible for the guilt suggests that an interpretation based on bolstering the subject's self-image may be the most productive. All of the subjects in these experiments had been asked to comply by engaging in some behavior which was of fairly high social desirability, that is, helping another person gather names for a petition. Because of this, compliance is likely to give a subject positive information about himself. It would be of interest in clarifying the processes involved in this guilt-compliance relationship to see if the same effect holds when the subject is asked to engage in some negative behavior. For example, are subjects who are made guilty likely to comply with another person who asks them to cheat?

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# SITUATIONAL INFLUENCE ON MORAL JUSTICE:

## A STUDY OF "FINKING"

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Piaget's notion of moral relativism with respect to peer-group standards was investigated by placing children in a situation in which they were questioned by an adult about their knowledge of a simulated transgression by a peer which they had witnessed earlier. Under these circumstances, every subject was willing to incriminate the guilty confederate. However, the presence of an innocent peer deterred subjects from making such accusations, which suggested an implicit peer norm against "finking" by a colleague. Furthermore, this norm applied selectively to reporting guilt of a high-status peer, with no restraint against a low-status peer. The results suggest that situational conditions affect overt manifestations of overt justice, which in turn advises caution in interpreting results of investigations that utilize different operations for assessing conscience.

Piaget has described the course of moral development in the child as a gradual progression from an absolute and rigid sense of justice (*moral realism*) to a flexible sense of equity which involves consideration of mitigating circumstances (*moral relativism*), suggesting that moral relativism dominates the moralistic judgments of the child by the time of preadolescence (Piaget, 1932). That peer-group standards of morality exert conditional effects upon the child's sense of justice has been suggested indirectly in several experimental investigations of conscience and moralism (Crane, 1958; Luria, Goldwasser, & Goldwasser, 1963; MacRae, 1954). Two studies by Durkin (1959a, 1959b), in which second-, fifth-, and eighth-grade children were questioned about their response to an event of aggression toward them by a peer, failed to show evidence of progression from absolutistic toward relativistic moral concepts. In interpreting her data, Durkin suggested that eighth-graders may be relatively more sensitive than younger children to peer-disapproval of "tattling" to an authority.

Questioning by adult authority with respect to transgression committed by a peer is a fairly common occurrence in a child's life. From an adult point of view, albeit somewhat oversimplified, the ensuing conflict in the child is whether to be moral (and "fink" on the peer) or not; failure to reveal the transgression may thus be conceived as "wrong." However, because of peer group pressure the child may conceive it "right" to deviate from adult moral standards. The conflict then becomes not whether or not to be moral—but how to decide between two contending moral principles: not to lie (or disobey), or not to "fink." In order to investigate some of the conditions which may temper the preadolescent's basic sense of "right" and "wrong," the present study was designed to place children in a situation in which they had knowledge of a moral transgression (theft) by a peer and were questioned by an adult about this knowledge. The influence of the guilty peer, presence of the guilty peer when the subject was questioned, and presence of other nonguilty peers when the subject was questioned were all investigated as conditions which might affect the child's willingness to report his knowledge of the transgression to an authority.

### EXPERIMENT I

*Subjects.* Subjects in this study were 48 middle-class children (27 boys and 21 girls) in two junior high school history classes. Their ages ranged from 12 years 6 months to 13 years 6 months, and they were all in either seventh or eighth grade. Two additional students (a boy and a girl) from the same history classes were employed as confederates.

*Procedure.* In order to simulate a realistic event of moral transgression, a contrived "theft" was arranged during regular classes. By arrangement, the teacher was called from the room to take an important telephone call. During her absence, a preinstructed confederate student arose from his seat ostensibly to dispose of his chewing gum into a wastebasket; as he did so, the student scooped up $.75 in silver coins which had been left on a small table at the front of the room, saying "Hey, look; how about that!" He pocketed the money and returned to his seat before the teacher reentered the room. Shortly after the teacher had returned and the classroom activities had been resumed, she received a prearranged call on the local intercom telephone and engaged in a cryptic conversation for a few seconds, repeating, "I don't know," several times. Somewhat later during the class, a messenger entered the classroom to request that students be called from the room one by one for interviews concerning casting a school talent show. As subjects left the room, either singly or in pairs, with the messenger, they were intercepted in the corridor by the experimenter and diverted into an empty office for the experimental interrogation.

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The interrogation itself consisted of a simple set of three direct questions, asked in a nonthreatening but business-like manner: (*a*) "Do you know whether someone took some change (about $.75) left near Mrs. X's desk today?" (*b*) "Do you know who took it?" (*c*) (If so) "Who took it?" Data were tabulated according to whether the first two questions were answered "yes" or "no," and whether the subject named the guilty confederate in response to the third question. Following the interrogation, the subjects were told that the entire situation was an experimental game, and they were sent to the playground to remain until the end of the period for a more complete explanation of the afternoon's events.

*Manipulation of variables*. Status of the guilty confederate was determined by means of a sociometric questionnaire administered approximately a week before the experiment itself. Subjects were requested to "list five people whom you consider worthy to represent the group at a planned discussion seminar, banquet, and dance for representatives from all of the schools in this area." The student receiving the greatest number of mentions, an eighth-grade girl, was selected as the high-status experimental confederate; the student receiving the lowest number of mentions (none), a seventh-grade boy, was selected as the low-status experimental confederate. These confederates played the role of the guilty thief in the simulation described above, one in each of two comparable history classes. For half of the subjects in each class, the guilty confederate was present during the interrogation; for the other half, the guilty confederate remained in the classroom and was absent during the interrogation. Half the subjects were questioned individually (apart from the presence or absence of the guilty confederate); the other half were interrogated in pairs, with the critical experimental subject accompanied by a second student from the same class (who was subsequently questioned in the same manner, but for whom data were not recorded). Subjects were debriefed at the end of the interrogation by being told that it was a game of "how to perceive people accurately." The role of the confederates was also revealed.

The three experimental variables thus generated eight combinations of situational conditions, with six subjects treated to each combination. (See Table 1.)

All three questions were asked of each subject, except when a "no" answer to an earlier question automatically implied negative answers to subsequent questions.

## RESULTS AND DISCUSSION

Affirmative answers indicate willingness of the subject to report his observation of the simulated theft to a questioning authority. Since the theft occurred openly before the entire class, an absolute sense of moral justice would be expected to lead subjects to answer all three questions truthfully in the affirmative. Table 1 summarizes the frequency of affirmative answers to each question under each of the eight experimental conditions. These data are self-demonstrative; however, Fisher exact-probability tests (Siegel, 1956) of differences between all possible comparison pairs reveal that on the first two questions, affirmative answers occurred significantly less frequently under the high-status thief/accused absent/questioned-in-pairs condition than under any other condition ($p <$ .05). With respect to the third question (naming the guilty party), affirmative answers occurred significantly less frequently in the high-status thief/questioned-in-pairs conditions (regardless of whether or not the guilty confederate was present) than under other conditions ($p < .05$).

Further specific differences with respect to admitting knowledge of the simulated theft were a function of whether or not the high-status guilty peer was or was not present during interviewing. When the high-status guilty confederate was *absent* during interrogation, none of the subjects interviewed admitted any knowledge of the theft, presumably because the circumstances would imply that there was no other existing evidence against the thief, so that the implicit peer norm against "ratting" on a colleague was exercised fully. In contrast, the presence of the high-status guilty confederate sitting quietly in a desk beside the experimenter during interrogation apparently suggested that some evidence might already exist to incriminate the thief. Under

TABLE 1

PROPORTIONS OF AFFIRMATIVE ("CONFESSION") ANSWERS TO THREE INTERROGATIONS ABOUT THE SIMULATED THEFT, ACCORDING TO EXPERIMENTAL CONDITIONS

| Question | Status of thief | Experimental condition | | | |
|---|---|---|---|---|---|
| | | Guilty thief present | | Guilty thief absent | |
| | | Questioned alone | Questioned in pairs | Questioned alone | Questioned in pairs |
| Knowledge of theft | High | 6/6 | 5/6 | 6/6 | 0/6* |
| | Low | 6/6 | 5/6 | 6/6 | 5/6 |
| Knowledge of guilty person | High | 6/6 | 5/6 | 5/6 | 0/6* |
| | Low | 6/6 | 5/6 | 6/6 | 5/6 |
| Name the thief | High | 5/6 | 1/6* | 4/6 | 0/6* |
| | Low | 6/6 | 5/6 | 5/6 | 5/6 |

* $p < .05$.

these conditions, most of the subjects were willing to acknowledge that they knew of the theft and even to admit that they could identify the thief. Only when subjects were brought to the third question, demanding that they stand before the guilty confederate and identify him by name, did the norm against "finking" on a high-status peer exercise an influence, leading most of the subjects interrogated under these circumstances to refuse to identify the thief directly.

It is important to call attention to the fact that this interpretation of differences in "finking" on high-status and low-status peers was subject to some confounding in the present study. The status differentiation of the two confederates was clearly established through the earlier sociometric investigation. The sex variable was presumably of little relevance to the perceived status of these people, since the high-status person (a girl) received equal numbers of nominations from boy and girl classmates, and since the low-status person (a boy) received no nomination at all from either sex. Furthermore, the distribution of boys and girls among the eight experimental subgroups was approximately equal, and there was no evidence that the behavior of boys and girls differed during the interrogation. Nevertheless, since the high-status confederate was a girl, and the low-status confederate a boy, it might be argued that differential application of the peer-group standard against "finking" may be a function not of *status* as such, but of *sex*. Conceivably, there are standards against "finking" against a girl which do not apply to the question of "finking" against a boy. Another possible artifact was that the different behavior of the two groups might have reflected a different classroom atmosphere, rather than the status of the thief. To clarify these issues, a second, similar experiment was conducted.

## EXPERIMENT II

*Subjects.* Subjects in this study were 56 middle-class boys whose ages ranged from 11 to 13 and who belonged to one of two youth groups attached to a suburban community center.

*Procedure.* The simulated event of moral transgression was arranged during the weekly meeting of each respective youth group. The adult advisor of the groups (in both cases the same individual) brought a tape recorder and put it on the table in front of her. In answer to queries, she told the group that the tapes were very important inasmuch as they included the program outline for various youth groups for the entire year. By arrangement, the advisor was then called from the room to take an important telephone call. The preinstructed confederate went ostensibly to dispose of his chewing gum into a wastebasket; as he did so, he went to the table, plugged in the tape recorder, and started to sing into the microphone in a clowning manner before unplugging the recorder, putting it back, and exclaiming: "I must have erased some of this tape!" The advisor then came back with her assistant and told the boys that she had to leave for about ½ hour. She left, taking the tape recorder with her. Shortly after, the assistant received a prearranged call and engaged in a cryptic conversation for a few seconds, repeating "I don't know" several times. Subjects were then called from the room, either singly or in pairs, under the same pretext (casting of a talent show) that had been used in Experiment I.

From this point on Experiment II follows closely the pattern of Experiment I. Status of the guilty confederate had been determined a week earlier by the same sociometric measure used in Experiment I. The interrogation also was essentially the same, consisting of the following three questions: (*a*) "Do you know whether someone fooled around with my tape recorder tonight?" (*b*) "Do you know who did it?" (*c*) (If so) "Who did it?" Data tabulation was identical to that in Experiment I, as was the debriefing procedure.

## RESULTS AND DISCUSSION

Table 2 summarizes the frequency of affirmative answers to each question under each of the eight experimental conditions. Fisher exact-probability tests (Siegel, 1956) of differences between all possible comparison pairs reveal that affirmative answers to the third question (naming the guilty party) occurred significantly less frequently in the two high-status thief/questioned-in-pairs conditions (regardless of whether or not the guilty confederate was present) than other conditions ($p < .05$).

The data from both experiments leave little doubt as to the fact that the simulated transgressions were clearly observed by the subjects and interpreted by them as moral transgressions. Under certain circumstances, every subject interviewed was willing to accuse and incriminate a guilty confederate. When questioned alone, a situation in which the manner of the adult interrogator implies an expectation of moral righteousness and truthfulness, almost all the subjects questioned were willing to confess their full knowledge of the transgression or even to identify the transgressor by name. However, the presence of a peer who also witnessed the simulated transgression appears to deter subjects from making such accusations and identification of the transgressor. The status of the guilty peer seems to be a crucial variable in this context: the implicit peer-group norm against "finking" seems to apply selectively to the question of reporting the guilt of a respected and esteemed classmate, with no evidence of restraint against reporting and identifying a low-status guilty peer. There was pronounced reluctance to identify the high-status transgressor directly by name regardless of whether the guilty person was or was not actually present during the interrogation, and regardless of the seriousness of the transgression (theft versus tape erasure).[2]

The data reported here represent only a limited definition of the particular kinds of situational conditions which may operate in conjunction with an individual's internalized

TABLE 2

PROPORTIONS OF AFFIRMATIVE ("CONFESSION") ANSWERS TO THREE INTERROGATIONS ABOUT THE SIMULATED TAPE ERASURE, ACCORDING TO EXPERIMENTAL CONDITIONS

| Question | Status of transgressor | Experimental condition | | | |
|---|---|---|---|---|---|
| | | Guilty transgressor present | | Guilty transgressor absent | |
| | | Questioned alone | Questioned in pairs | Questioned alone | Questioned in pairs |
| Knowledge of recorder manipulation | High | 7/7 | 7/7 | 7/7 | 5/7 |
| | Low | 7/7 | 7/7 | 7/7 | 7/7 |
| Knowledge of guilty person | High | 7/7 | 5/7 | 7/7 | 5/7 |
| | Low | 7/7 | 7/7 | 7/7 | 7/7 |
| Name the transgressor | High | 7/7 | 1/7* | 5/7 | 0/7* |
| | Low | 7/7 | 7/7 | 7/7 | 7/7 |

* $p < .05$.

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[2] It is assumed that those who deny knowledge of a transgression and/or transgressor will also refuse to identify the transgressor. Still, the conservative reader might argue that the conflict between obeying and informing can be analyzed only from responses to the third question, provided that all those who responded negatively to the first two questions are eliminated as subjects. When this was done, the results remained practically unchanged: in Experiment I, the high-status thief/questioned in pairs/guilty-thief-present condition still yielded significantly less ($p < .05$) affirmative answers than any condition involving the low-status transgressor; in Experiment II, there was no change at all.

"conscience" or sense of moral justice. The situation in both experiments differed somewhat as to the nature of the experimenter-subject relationship and the seriousness of the transgression. Furthermore, it can be argued that naturalistic classes (groups to which the high- or low-status transgressors belonged) rather than individual subjects constituted the units of sampling. The possibility that the tendency to lie or to inform was due to initial differences between these classes therefore exists. Nevertheless, the present study showed the general effect of status variation to hold constant across two experiments of somewhat different type, thus minimizing the possibility of unwarranted generalization. In the long run, however, it may be more advisable to employ ad hoc groups in the laboratory in order to allow the manipulation of the status of the transgressor, while at the same time ruling out the likelihood of any more than random differences between the groups to which the transgressors belong.

Finally, a word of caution. The risk of cost to the subject (e.g., loss of self-esteem to the informer when the deception is revealed) in this type of study cannot be ruled out. In the present study, subjects' reaction during the comprehensive debriefing procedure, as well as reports by the adult authorities involved, seemed to indicate otherwise. Evidently, the entire episode was subsequently viewed as an amusing game, devoid of any dramatic or anxiety-producing aftereffects. As a rule, however, studies involving manipulations in areas such as group pressure, divided loyalties, or informing on malefactors almost inevitably run the risk of inflicting some costs on the subjects involved. Every effort should be made to keep such costs at a minimum.

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# REINFORCING BEHAVIOR OF "NAIVE" TRAINERS[1]

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An experiment was performed to determine whether the sanctioning behavior of naive trainers is consistent with principles of behavior modification, that is, responsive to a trainee's performance alone, or whether it is influenced by variables other than performance. Two variables which may affect the administration of rewards and punishments are the perceived competence of the trainee and difficulty of the task. These two variables were manipulated in a 2 × 2 factorial design with easy (E) and difficult (D) tasks and competent (C) and noncompetent (NC) trainees. The trainers were responsible for administering rewards and punishments to trainees while teaching them a concept task; use of a confederate enabled identical trainee performance for all conditions. The data indicate that extraperformance variables do affect the reinforcing behavior of naive trainers as predicted by attribution theory. The results provide behavioral confirmation for predictions from the theory which has been primarily tested using perceptual judgmental data.

Some "psychologically sophisticated" training agents, when attempting to modify behavior, would administer reward (and possibly punishment) contingent upon the performance of the trainee relative to an established criterion level. The motivational state of the subject might be considered in selecting the type of reinforcement but otherwise would be irrelevant to administration of the reinforcement schedule. Equally irrelevant would be consideration of the intentions, attitudes, and abilities of the trainee or the affective state of the trainer.

Independent of the normative issue of whether such a procedure would be effective or efficient, one may reasonably ask, "Is the reinforcing behavior of naive trainers consistent with this behavioristic tenet or are naive trainers responsive to variables other than performance per se?" Cursory observation suggests that socializing agents indeed are influenced by factors other than performance. A mother is unlikely to punish her 2-month-old child for throwing his food dish, whereas at 8 months of age punishment would be probable. Teachers often praise the mediocre performance of one pupil because he "tried hard" while the identical behavior by another is criticized because of "poor effort." But if performance does not solely determine a trainer's sanctioning behavior what are the other controlling variables?

At least two plausible alternatives present themselves:

1. The administration of rewards and punishments reflects the trainer's affective state and follows a principle of reciprocity; reward (or punishment) is administered when the trainee behavior is rewarding (or punishing) to the trainer, the amount of reinforcement administered being equated with that received. Thus variables which affect the satisfaction or dissatisfaction the trainer experiences would be expected to modify the way reinforcements are administered.

2. The trainer responds not to performance per se but to inferences about relevant dispositional properties of the trainee, that is, to inferred intentions, motivations, etc. Whether reward or punishment is used and the amount of reward or degree of punishment employed depend on the "meaning" of performance; the trainer is basically concerned with modifying dispositional states, not performance per se. Poor performance which is attributed to low motivation, lack of eagerness, inattention, and low effort is punished, whereas poor performance attributed to lack of ability or difficulty of the task is not punished. Thus variables which affect "attribution processes" would be expected to

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modify the trainer's use of rewards and punishments.

These presumed determinants of sanctioning behavior may be interrelated in the sense that the response to frustration will depend on the perceived "cause" of the behavior producing the frustration. As Jones and deCharms (1957) have pointed out,

> Subjectively . . . when the locus of phenomenal causality is perceived as internal to the agent of frustration, negative evaluation is more severe. . . . the perceived reason for (an individual's) failure plays an important role in any concurrent evaluation of him. . . . if the (individual) is perceived to fail as a function of inadequate ability, there should be less of a tendency to evaluate him negatively than if his failure is motivational in origin. One's competence, by and large, is an attribute cluster over which the individual has little control—the sources of competence are in a sense external to the sphere of individual autonomy and responsibility. Motivation, on the other hand, is an attribute cluster likely to be perceived as self-caused or internally controlled by the behavior in question [p. 79].

The strength of negative evaluation upon which the punitiveness of sanctions is presumably contingent should thus depend on the extent to which a trainee's performance is poorer than expected (and his "failure" is frustrating to the trainer) and the degree to which the perceived cause of inadequate performance is attributed to factors over which the trainee has some degree of control (e.g., motivation, interest, effort).

The present experiment investigates the effects of two variables, subject competence and task difficulty, on the reinforcement behavior of "naive" trainers as they attempt to teach a concept task. Trainers have available responses graded in reward or punishment value which are administered after each response of a trainee. The concept task being taught is either easy or difficult and the trainee is identified as either highly competent or incompetent at learning concepts. The trainee is a confederate of the experimenter and, in all conditions, follows the same fixed schedule of responding.

The sanctioning behavior of trainers who employ the "performance reinforcement" principle of behavior modification should not be influenced by considerations of the trainee's competence or the difficulty of the task, and for such trainers one would expect reinforcement to be administered in similar ways under all experimental conditions. If, however, the trainer is responsive to his own level of frustration in conjunction with inferences about the motivational orientation of the trainee, one might expect the administration of rewards and punishments to vary depending on experimental conditions.

The trainer's degree of frustration should be a function of his goals and the extent to which the trainee's performance blocks goal attainment. In the present experiment the trainer's goal was presumably to "win the prize" for having the best performing trainee; the trainee is evaluated, however, only with respect to others in the same ability category and for the same task difficulty level. This within-condition evaluative set should weaken and possibly eliminate differences in level of aspiration associated with experimental conditions as a potential source of differential frustration, but leave relatively high the probability that under all conditions trainers would experience some frustration.

The response to this frustration should, however, depend on whether the trainee's poor performance is perceived as a result of external factors or internal, nonmodifiable variables. Jones and Davis (1965) and Kelley (1967) have discussed the criteria that lead to an inference of intention when an observed effect produced by an individual deviates from an expectation:

> intention, as a reflection of a deliberate act of choice to produce the observed effect is reflected in deviation of the observed effect from those attributable to situational demands and personal characteristics of the actor [Kelley, 1967, p. 211].

In the present context it is plausible to assume that poor performance in an easy task as contrasted to a difficult task, and by a competent as compared to a noncompetent trainee, is more likely to be perceived as "intentional" and thus negatively evaluated and responded to more punitively. Further, competency of the trainee and task difficulty should interact; a competent person who performs poorly on an easy task should be reacted to most punitively, whereas a noncompetent person who fails on a difficult task should be responded to least punitively.

## METHOD

### *Experimental Design*

Forty male volunteers from the introductory psychology course at Dartmouth College were given the ostensible task of training a fellow student on a concept-formation task. The student to be trained was a confederate, and his performance was predetermined and identical for all trainers. The trainer was set to expect a certain level of performance by his "subject" by giving him instructions pertaining to the difficulty of the task and the competence of his subject. A 2 × 2 factorial design was employed with two levels of task difficulty (easy, E, and difficult, D) and two levels of subject competence (competent, C, and noncompetent, NC). The four conditions were thus EC, ENC, DC, and DNC. Assignment of trainers to the four conditions was random upon appearance at the laboratory, with 10 trainers assigned to each condition.

### *Apparatus*

The apparatus consisted of a 2 × 4-foot wooden screen separating the trainer from his subject. In the center of the screen was a slot through which the stimulus cards were presented. On the trainer's side of the screen was a panel containing five "reinforcement" switches with a colored light beside each switch. The switches were placed vertically and labeled, from top to bottom, "high shock," "low shock," "neutral," "low reward," and "high reward," respectively. On the table in front of the trainer was another panel consisting of four lights labeled with the names of the four groups into which the stimulus cards were to be sorted.

On the confederate's side of the screen was a vertical panel of three lights and two counters. The lights were labeled "high shock," "low shock," and "neutral," and the counters, "low reward" and "high reward." All were operated by the reinforcement switches available to the trainer. A second panel in front of the confederate consisted of four "response" buttons which operated the four "answer" lights in front of the trainer. A pair of finger electrodes used to deliver the shock was also on the table. These electrodes were connected in parallel to a 6-volt battery and two inductoria. By adjusting the inductoria, two levels of shock could be delivered and demonstrated to the trainer. The levels were those established in pretesting as having a scale value of 2.5 and 8 of painfulness when rated on an 11-point scale.

The stimulus cards were 12 photographs depicting four basic facial expressions: smiling, surprise, doubt, and anger. In the easy task situation, the discrimination that the trainer was supposed to teach his subject was based on the four expressions. In the difficult task, the discrimination was based upon a series of small ink spots in the corners. The spots were almost imperceptible unless pointed out, so the task appeared extremely difficult. On the back of each card was the name of the group to which the card belonged, as well as a number from 1 to 12. These numbers were used by the trainer to present the cards in a random order according to a schedule provided by the experimenter.

### *Procedure*

Upon arrival at the laboratory, the trainer was seated in front of the screen and the following instructions given:

> This is an experiment dealing with the learning of concepts. A concept, in case you are not familiar with the term, may be something defined socially, such as dogs or cats, etc., or it may be something defined arbitrarily, such as red circles or blue squares. These, of course, would have to be learned. We are in the early stages of developing a new method of training for such a task, and in a few minutes a sophomore student who has already taken a number of tests to measure his ability for learning concepts will be here, and your job is going to be to teach him this task, which I will explain to you now.
>
> In front of you you will see 12 cards divided into four groups of 3 cards each. These four piles define the concepts you will try to get your subject to learn. Now such a task can be made either very difficult or very easy, depending upon what the basis of the discrimination is.

### *Easy Task Groups*

> The discrimination or concept that you are going to have to teach your subject is really quite simple, and if you look at the cards I think that you will readily see that it is based upon or has something to do with the expressions on the faces. Now I want you to tell me in your own words how you think the four groups differ [trainer verbalized his version of the discrimination with prompting from the experimenter so that he got it readily and the task was made to appear simple].

### *Difficult Task Groups*

> The discrimination or concept that you are going to have to teach your subject is really quite difficult, and if you look at the cards I think you will understand what I mean. I want you to tell me in your own words how you think the four groups differ [trainer attempted to verbalize his version of the discrimination with many errors and negative responses from the experimenter until the experimenter finally had to show him what the discrimination was].
>
> Okay, now for the apparatus setup. First of all, you will present the cards one at a time for approximately 10 seconds through this slot here. The cards will be presented in random order according to this schedule in front of you. Your subject has four buttons over on his side, one for each category. They will light up one of these four lights marked VOX, MIJ, ZIT, and BAM. When he has made his answers, you will have the option of five responses. The switches on this box are the way you will make these responses. The top

switch will deliver a fairly strong shock, the second a mild shock, the third is neutral and just lights a small light, the fourth gives him a small reward, and the last switch will give him a large reward. [The trainer was then taken around to the confederate's side of the table. The values of the two reward levels were explained, 5¢ and 2½¢ for high and low, respectively, and the two levels of shock demonstrated. The trainer then returned to his own seat.]

Now, to make it more interesting for the trainers, since your subject is going to be working for money, we are going to give $5 to the trainer who does the best job of getting his subject to learn the task. This will be measured by the number of right responses on the last trial or by the number of trials it takes to reach a criterion of two errorless trials. The task will be stopped after 10 trials whether or not your subject has completely learned the discrimination.

### *Competent Trainees*

I should tell you a couple of things about your subject. As I said earlier, several of the sophomore classes were tested for their ability to do this type of task. Since we are developing a new method of training, we want to compare our results with written tests. We have selected a group of subjects who scored very high on the tests as well as another group who scored very low, and we are now testing them on our task to see if they differentially score high and low as on the written test. Your subject, let me see now [the experimenter consulted a list of names], scored very high on the written tests so that he should have an easy time with this task. For the $5 we will compare your results only with trainers who also had easy subjects.

### *Noncompetent Trainees*

I should tell you a couple of things about your subject. As I said earlier, several of the sophomore classes were tested for their ability to do this type of task. Since we are developing a new method of training, we want to compare our results with the written tests. We have selected a group of subjects who scored very high on the tests as well as another group who scored very low, and we are now testing them on our task to see if they differentially score high and low as on the written test. Your subject, let me see now [the experimenter consulted list of names], scored very low on the written tests so that he will probably have quite a bit of difficulty with this task. For the $5 we will compare your results only with those other trainers who also had difficult subjects.

Now, just a couple of things to remember. The use of the five different responses is entirely up to your best judgment; the only thing you should try to do is to get him to learn the discrimination as quickly as possible. I also want to tell you that the order of his buttons, that is, from left to right, is not necessarily the same as your order of lights so do not try to give him cues as to position of the lights or you will only confuse him.

Okay. Are there any questions? While I explain the task to your subject you can get the cards in order for the first trial.

About the time instructions to the trainer were completed, the confederate arrived at the laboratory and was asked to come in and take a seat in the chair on the opposite side of the screen to the trainer. He was given "instructions" as to the nature of his task, the values of the rewards and punishments, etc.

Unknown to the trainer, of course, the confederate was never actually connected to the shock apparatus. Also unknown to the trainer was the fact that the confederate was given a card with the "responses" he was to make to every stimulus card. Thus every trainer, regardless of his subject's competence and the task's difficulty, had the same response given to each stimulus card.

A trial consisted of one complete presentation of the 12 stimulus cards, and the complete task consisted of 10 trials. The confederate thus made 120 responses. At the end of each trial, the trainer was given time to order the cards for the next trial. The response schedule used by the confederate was constructed in such a way as to give the appearance of a slow learner's "learning curve." The schedule was as follows, with the first number of each pair as the trial number and the second, the number correct on that trial: 1,1; 2,1; 3,2; 4,2; 5,4; 6,4; 7,5; 8,5; 9,6; 10,6.

No verbal communication was allowed during the experiment, and the confederate was instructed to keep his interaction with the trainer to a minimum before the experiment. For this reason he did not arrive at the laboratory until the trainer was inside receiving instructions. This prevented the trainer from obtaining any incidental cues as to the confederate's competence or motivation.

If there were no further questions, the experiment was begun. The experimenter sat to the left of and slightly behind the trainer in order to record his responses. For each stimulus card, a record was made of the type and level of reinforcement employed by the trainer.

After completion of the 10 trial blocks the trainer was asked to complete a questionnaire rating the following items on an 11-point scale: (*a*) difficulty of the task, and (*b*) competence of your "pupil."

## RESULTS

### *Subjective Check on Experimental Inductions*

The mean scores on the two items of the questionnaire relevant for assessing the trainers' perceptions of the conditions are shown in Table 1. Task difficulty was rated significantly higher in the "difficult" task conditions ($p < .005$, $F = 10.06$, $df = 1/18$), and ratings of trainee competency were similarly consistent with experimental treatments ($p < .001$, $F = 28.54$, $df = 1/18$). Although the interaction was not significant there is some indication that trainers differentially estimate the difficulty of the "easy" task depending upon the perceived competence of the trainee; failure by the competent trainee appears, in part, to be accounted for by attributing greater difficulty to the task. Noncompetent trainees are also seen as slightly more competent when working on the difficult task than on the easy task, although again the interaction of competency and task difficulty is not significant. It is clear, however, that trainers' evaluations of the task and their trainee's ability were as intended by the experimenter.

TABLE 1

MEAN RATINGS OF THE QUESTIONNAIRE ITEMS BY THE FOUR EXPERIMENTAL CONDITIONS

| Item no. | Competence of trainee | Task difficulty | |
|---|---|---|---|
| | | Easy | Difficult |
| 1. Difficulty of task | Competent | 7.25 | 8.40 |
| | Noncompetent | 6.70 | 8.35 |
| 2. Competence of "pupil" | Competent | 6.40 | 6.25 |
| | Noncompetent | 3.80 | 4.65 |

Note.—Higher number indicates greater difficulty and competence.

### *Reinforcement Behavior*

The confederate made 36 correct and 84 incorrect responses over the 10 trials. Correct and incorrect answers were analyzed separately. For correct responses the trainer almost invariably replied with high reward. There were some interesting deviations, however. Though not reliable statistically, there was a tendency for the trainers with NC subjects to give somewhat less high reward (HR), and consequently more low reward (LR), on both the easy and difficult tasks. The mean numbers of HR and LR used under the four conditions are presented in Table 2.

TABLE 2

MEAN NUMBER AND PERCENTAGES (IN PARENTHESES) OF HIGH REWARD (HR) AND LOW REWARD (LR) GIVEN FOR CORRECT RESPONSES UNDER THE FOUR EXPERIMENTAL CONDITIONS

| Type of reinforcement | Condition | | | |
|---|---|---|---|---|
| | EC | ENC | DC | DNC |
| HR | 33.6 (93.3) | 30.6 (85.0) | 33.1 (91.9) | 29.2 (80.6) |
| LR | 2.4 (6.7) | 5.4 (15.0) | 2.9 (8.1) | 6.8 (19.4) |
| Total | 36.0 | 36.0 | 36.0 | 36.0 |

Note.—Abbreviations: EC = easy task, competent subject; ENC = easy task, noncompetent subject; DC = difficult task, competent subject; DNC = difficult task, noncompetent subject.

For the incorrect responses, percentage use of the four levels of reinforcement is presented in Table 3. Shock is clearly the preferred response to an incorrect choice and high shock is used about 25% of the time. Weighted scores were calculated for each trainer by multiplying the total number of times he used low reward (LR), neutral (N), low shock (LS), and high shock (HS) by 1, 2, 3, and 4, respectively. An analysis of variance was then performed on these weighted scores. The main effects of task difficulty and trainee competence were highly reliable ($p < .001$), the $F$s being 124.82 ($df = 1/36$) and 14.97 ($df = 1/36$), respectively. The Competence × Difficulty interaction was marginally significant ($p < .06$, $F = 3.67$, $df = 1/36$). Figure 1 presents the Competence × Difficulty interaction and Table 3 the means of the weighted scores as a function of the condition and type of reinforcement.

The trials effect was analyzed separately for each condition as follows: On each trial, the proportion of each of the four reinforcement levels used for incorrect responses was calculated. These proportions were then subjected to an analysis of variance. In all four conditions, the main effect of levels (of reinforcement) was statistically reliable ($p$'s <

TABLE 3

MEANS OF WEIGHTED SCORES AND PERCENTAGES (IN PARENTHESES) FOR FOUR TYPES OF REINFORCEMENT GIVEN TO INCORRECT RESPONSES

| Type of reinforcement | Condition | | | |
|---|---|---|---|---|
| | EC | ENC | DC | DNC |
| LR | 4.8 (6.4) | 3.2 (3.8) | 8.0 (8.5) | 9.3 (11.1) |
| N | 31.6 (18.8) | 43.4 (25.8) | 47.2 (28.1) | 44.4 (26.4) |
| LS | 113.4 (45.0) | 106.8 (42.4) | 93.6 (37.3) | 124.8 (49.5) |
| HS | 100.0 (29.8) | 94.0 (28.0) | 84.8 (25.2) | 43.6 (13.0) |
| Total | 249.8 | 247.4 | 233.6 | 222.1 |

Note.—Abbreviations: see Table 2.

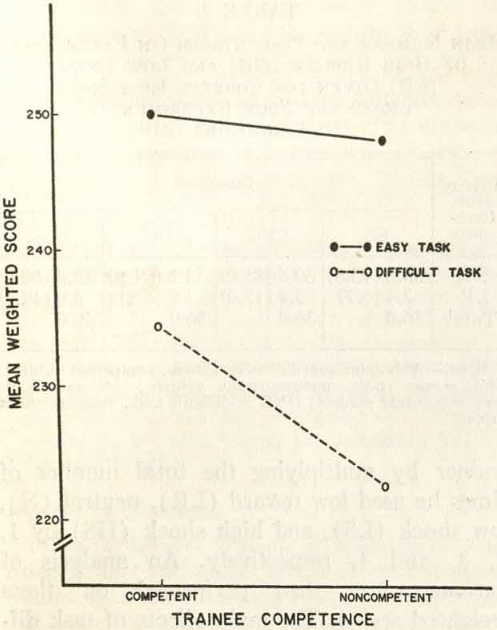

Fig. 1. Mean weighted scores of punitiveness of reinforcement as a function of experimental conditions (high score = greater punitiveness).

.001). The main effect of trials was not significant since summing across levels yielded a straight line parallel to the Y axis. The Levels × Trials interaction was dependable only for the EC condition ($p < .05, F = 1.65, df = 27/360$). Because LR and N appeared to account for only a small portion of the Levels × Trials interaction, a second analysis of variance was performed on each of the four conditions using only the LS and HS data. Once again the main effect of levels was statistically dependable for all four conditions ($p$'s $< .05$). For the EC condition the interaction $F$ ratio of 3.44 was statistically reliable ($p < .001, df = 9/180$). That for the DC condition was marginally significant ($p < .07, F = 1.80, df = 9/180$). Figures 2 and 3 present the Levels × Trials data for the EC and DC conditions, respectively. The curves for the other two conditions are similar in the sense that the number of HS responses increases and that of LS decreases over trials, but they differ in that the rate of change is much slower and the HS and LS curves never cross.

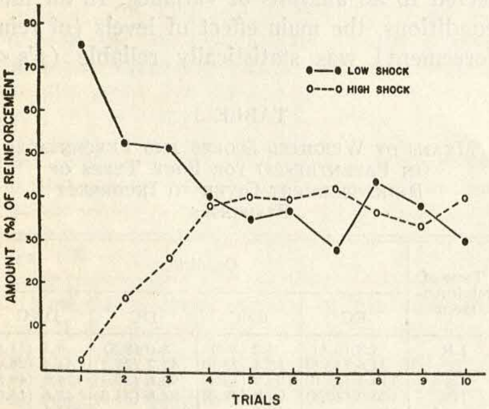

Fig. 2. Percentage use of high and low punishment over trials for the EC condition.

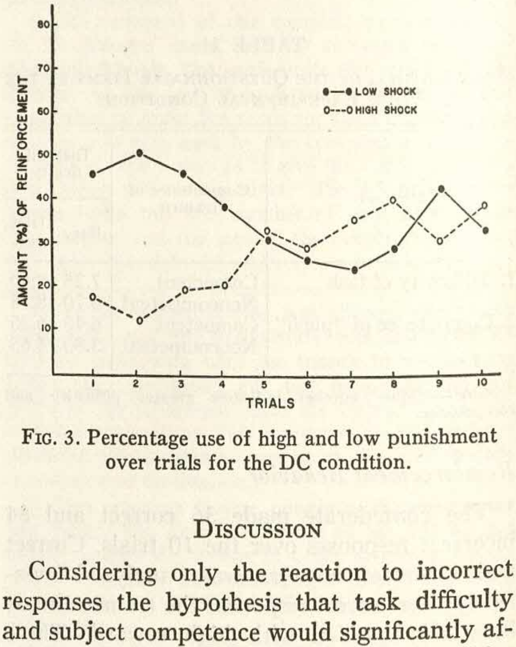

Fig. 3. Percentage use of high and low punishment over trials for the DC condition.

## Discussion

Considering only the reaction to incorrect responses the hypothesis that task difficulty and subject competence would significantly affect type and level of reinforcement used by the trainer was supported. Both main effects of difficulty and competence were highly reliable statistically. Thus, for a difficult task, the trainer tends to use a lower level of negative reinforcement than when the task is easy. That is, he uses low shock as opposed to high shock and, in addition, uses more neutral and low reward responses. Exactly the same effect is obtained for trainee competence. The trainer administers more low shock and more low reward and neutral responses for a noncompetent subject than he does for a competent subject. The Competence × Difficulty interaction, although only marginally reliable ($p < .06$), indicates that trainee competence is less important as a factor when the task is easy than when it is difficult. Perhaps the easy task is perceived as so easy that even a noncompetent subject should do reasonably well. As predicted, the most punitive responses are administered to trainees in the easy-competent condition and the least punitive to trainees in the difficult-noncompetent treatment.

The results are consistent with the theoretical rationale presented in the introduction. For all conditions, the generally poor performance of the trainee is frustrating to the trainer since it presumably reduces his chances of obtaining the prize "for being the most effective trainer." The response to the frustration is, however, dependent on the presumed cause of the trainee's poor prformance. Given the information that the task is difficult and the subject incompetent, failure is attributed by the trainer to factors over which the trainee has no control, and consequently is not responded to with a high level of punishment. On the other hand, given an easy task and a competent trainee, the trainer expects a high level of achievement, and poor performance is inconsistent with this expectation. Since the known facts of the situation do not account for the failure, the trainer must attribute it to some other cause. As Jones and Davis (1965) remarked, the trainer must ". . . find *sufficient reason* why the person acted . . ." and attributing to the subject a lack of motivation, desire, or attention provides a plausible explanation of the unexpected poor performance.

"Intentional" failure should not only be reacted to more negatively than "nonintentional" failure but could also motivate behavior that might effect a change in the responsible factors. Thus, if poor performance is attributed to a lack of motivation, for example, the appropriate action would be to try to increase motivation. The use of a higher level of punishment might be perceived as one way of increasing motivation. Another might be to increase the level of reward for correct responses. The tendency in the present study for trainers with competent subjects to give more high reward for correct responses and more high punishment for incorrect responses would support this notion.

The results of the analysis for trials also supports an "attribution-frustration" hypothesis. The Levels × Trials interaction for HS and LS was reliable only for the competent-subject conditions (Figures 2 and 3). The trainer was apparently set for a reasonable performance by his subject and for the first few trials used considerably more low shock than high. As it became apparent that the subject was not performing as expected the trainer began to increase the use of high shock until he was using more high shock than low. In the NC conditions, this interaction (Levels × Trials) did not occur. The trainer was not expecting a high level of performance and consequently maintained fairly stable levels of both high and low shock, with more low shock than high.

The overall high proportion of shock responses to incorrect choices presumably reflects either a general norm that punishment is an effective procedure for inducing behavioral change or a general bias to perceive persons as primary "causes" of failure. As Heider (1944) has noted,

> there probably exists a tendency to be "intropunitive" in regard to other persons, that is, to see the cause of their successes and failures in their personal characteristics and not in other conditions [p. 361].

Rosenthal and Jacobsen (1968) have presented results indicating that creating for teachers higher expectations about the ability of pupils enhances student performance, but the factors mediating the improved performance were not identified. Possibly, like the "naive" trainers in the present experiment, experienced trainers differentially administer rewards and punishments depending on the characteristics attributed to pupils. Although the present study provides no information on the effectiveness of the reinforcement schedules used by the trainers, there is some indication that trainees who are presumed to have low ability as compared to those with high ability receive less differentiated feedback, that is, fewer high rewards for correct responses and fewer high punishments for incorrect responses. One would surmise that such a schedule, providing as it does fewer discriminable cues as to performance adequacy, leads to slower learning. Thus a trainer's expectations of low ability may have the character of a "self-fulfilling prophecy."

The present results strongly support the assumption that situational factors and personal characteristics of a trainee influence the way in which rewards and punishments are administered; trainers are clearly sensitive to variables other than performance per se in selecting type and level of reinforcement. Milgram (1964) has dramatically demonstrated that social pressure can modify the level of punishment employed by a trainer, and Dustin and Davis (1967) have recently reported that personality characteristics of a trainer also influence sanctioning behavior. No doubt, further research will identify still other broad categories of variables that affect the behavior of socializing agents and contribute further to an understanding of the psychology of social reinforcement.

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# EFFECTS OF LIVE AND SYMBOLIC DEVIANT-MODELING CUES ON ADOPTION OF A PREVIOUSLY LEARNED STANDARD[1]

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Two experiments are reported which examined the relative influence of live and symbolic deviant-modeling cues on children's adoption of a previously learned self-reward standard. The heuristic concept of information value was introduced, and predictions were based on the presumed difference in the information value of live- and symbolic-modeling cues. In Experiment I, as anticipated, exposure to deviant live modeling weakened adoption of the previously learned standard more than did exposure to deviant symbolic modeling, although both treatments had a significant effect. Further, the two cues appeared to operate additively. Experiment II replicated, in a multiple-modeling situation, the finding of Experiment I regarding the relative potency of live and symbolic deviant cues and suggested that their differential influence is not affected by the additional presence of live or symbolic nondeviant cues. Results and implications are discussed in terms of both the empirical findings themselves and the presumed information value of the cues provided.

Most formal socialization practices involve direct instruction to abide by a variety of stringent social rules. Thus, in our culture, children are often admonished by parents and teachers to be truthful, to be fair in their dealings with others, and to put forth maximal effort in achievement situations. The purpose of such training is presumably to encourage later adherence to such standards when the individual is free to regulate his own behavior and is not constrained by the presence of external agents. In fact, a person's adoption and enforcement of self-regulatory standards is often taken as a critical index of the effectiveness of the socialization practices to which he has been exposed.

Nonetheless, both children and adults may forsake their training or "lower their standards" in a variety of ways, particularly when high-magnitude incentives are at stake. A growing body of social learning research suggests that one type of social experience which may partially account for the variance in subsequent rule adoption is observation of the self-imposed standards of others. It has been shown, for example, that the adoption of a self-reward criterion by children may be a joint function of the direct instruction which they receive and the behavioral standards which they actually see displayed by social agents, and that observing a deviant other is especially pernicious (e.g., Bandura, Grusec, & Menlove, 1967; McMains & Liebert, 1968; Mischel & Liebert, 1966, 1967; Ross, 1962). However, despite many experimental demonstrations, an explicit conceptual framework for understanding the phenomena under study has not yet emerged. The present paper suggests an informational analysis of the influence of deviant modeling cues on adherence to previously learned rules and reports two new experiments which, guided by this formulation, extended the experimental exploration to an examination of symbolic forms of modeling.

Walters and Cheyne (1966) suggested that verbal structuring of a prohibition (i.e., "cognitive structure") serves as an effective deterrent to violation because of its information value for children who have already been trained in rule adherence. They reasoned that such structuring supplies information about the social acceptability or unacceptability of various modes of behavior, an interpretation which has considerable content validity in

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view of the communications used.[2] The present authors further posit that mere observation of the behavior of others provides the same type of information. Consonant with the latter suggestion, recent experimental evidence indicates that children will readily adopt even quite stringent standards if such are consistently preached and practiced (e.g., McMains & Liebert, 1968). However, when uniform social acceptance of a rule is not apparent, as is the case after observing a deviant peer or adult, a stringent standard may be eschewed (e.g., Bandura, Grusec, & Menlove, 1967; McMains & Liebert, 1968).

It may be inferred from such findings that one critical determinant of children's self-imposition of a rule is the degree to which they see adherence to it as being widely accepted or expected and thus conclude that it is necessarily *applicable to them.* Further, if it is assumed that the perceived applicability of a rule influences its future adoption by an observer, then a general heuristic principle may be outlined for predicting the influence of variations in deviant-modeling cues. Specifically, exposure to deviant-modeling cues is cast as the provision of information that a rule (which has been previously taught) may not be self-imposed by all persons. Such information, it is further posited, may then lead observers to the conclusion that the rule in question is not necessarily applicable to them. Moreover, whereas adherence to generally applicable rules has many learned implications with respect to both short- and long-term consequences, most children have also learned by school age that rules of questionable applicability, such as those which are regularly broken by others, may be circumvented with reasonable safety.

Although a person can rarely establish with complete certainty that a rule is unequivocally applicable (or not applicable) in a given situation, there are many instances in which a decision regarding rule applicability must be made prior to action. To aid in the determination, the value placed upon available information concerning others' adoption of the rule is crucial. As used in the present formulation, *information* "applies to facts that are gathered in any way, as by reading, observation, hearsay, etc., and does not necessarily connote validity (inaccurate *information*) [Webster's, 1953, pp. 749–750]," and *value* refers to "that quality . . . according to which (something) is thought of as more or less desirable, useful, estimable, important, etc. [Webster's, 1953, p. 1609]." Thus, as applied to modeling cues, *information value* refers to the perceived utility of cue-provided signs in documenting the applicability of a rule for guiding one's own actions.

Information value of modeling cues is viewed as a heuristic concept which may have widespread application and, as such, may be linked with a variety of potentially manipulable variables. With respect to the studies cited previously, the value of information about others' deviation from a socially prescribed rule may be affected by the extent to which the information is perceived as reflecting stable rather than accidental or chance occurrences. In this regard, it is posited that the type (or source) and number of available modeling cues influence information value.

In previous experiments which have investigated the effects of deviant modeling cues upon rule adherence, only live exposure to the behavior of others has been employed. However, in life situations deviant-modeling cues are frequently provided indirectly (e.g., through verbal or written reports) as well as by direct observation. For example, in the course of socialization there are numerous occasions in which a child hears or reads about others' rejection of a socially prescribed rule, a phenomenon which may be thought of as *symbolic modeling*. Such indirect forms of observation provide only after-the-fact data, and their information value is thus reduced because of the possibility of forgetting, misunderstanding, or deliberate misrepresentation by the informant. It is therefore reasoned that a deviant symbolic-modeling cue has less information value than would direct observation of the reported behavior as it occurred and that, as a result, symbolic cues should have a less powerful effect on rule adoption than live-modeling cues.

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[2] Examples of previous "cognitive structure" manipulations include "No, that's for older boys [Aronfreed & Reber, 1965]" and "Now these toys have been arranged for someone else, so you'd better not touch them. If you are a good boy (girl) and do not touch the toys, we can play a game together in a little while . . . [Parke, 1967]."

A second aspect of the information provided by deviant-modeling cues, regardless of whether they are live or symbolic, is the consistency with which a rule has been rejected by others. The more internally consistent modeling cues there are, the more available information there is concerning the stability of the deviant behavior. It is therefore assumed that (*a*) multiple cues providing consistent information relevant to the violation of rules by others increase information value and (*b*) such multiple cues operate additively.

The two experiments which follow investigated the joint influence of direct instruction and deviant-modeling cues in light of the informational analysis presented previously. The twofold purpose of Experiment I was to examine the relative effects of deviant live- and symbolic-modeling cues, as well as the effects of combining such cues, on children's adherence to a stringent self-reward standard which they were previously taught by another person. Experiment II was designed to examine the relative potency of symbolic and live exposure to violation of a stringent rule in conjunction with symbolic or live adherence to the rule by a second model, thereby more closely paralleling the complexities of "real life."

## EXPERIMENT I

Previous research has suggested that deviant standards which are exhibited by live models may significantly decrease children's willingness to abide by a more stringent standard which they have been directly taught (McMains & Liebert, 1968). The present experiment was designed to compare the deleterious influence of deviant live- and symbolic-modeling cues and to determine whether these variables interact.

Based on the informational analysis, it was predicted that observation of deviant (i.e., self-lenient) live-modeling cues would weaken stringent rule adoption more than would exposure to deviant symbolic-modeling cues. Additionally, it was anticipated that the effects of deviant live- and symbolic-modeling cues, the combination of which increases the amount (and thus the value) of information concerning rule violation by the model, would be additive.

A 2 × 2 factorial design, with the presence or absence of deviant symbolic-modeling cues and the presence or absence of deviant live-modeling cues, was employed. In the three modeling groups employing the same adult model, the experimenter explicitly told the child, in the presence of the model, that a stringent self-reward standard was to be used when performing the task. Subsequently, the subject was exposed to the model's self-lenient behavior which was in clear violation of this rule.[3] Since children in the no-model group were also instructed about the use of the stringent standard, it was possible to readily assess the negating effects of the various modeling conditions.

### *Method*

*Subjects and experimenters.* The subjects, 12 boys and 12 girls from the third and fourth grades of an elementary school in the Nashville area, were randomly assigned to four treatment groups. The same adult female was the experimenter for all subjects, and an adult male served as the model.

*Apparatus.* The apparatus was a bowling game similar to the one used by Liebert and Allen (1967). It consisted of a miniature bowling alley with a 3-foot runway, at the end of which were 10 score lights arranged in the wedge-shaped display used in commercial bowling alleys. The score of 5 appeared four times in the first row, the score of 10 three times in the second row, the score of 15 twice in the third row, and the bottom row consisted of a single 20. In addition to the score lights, a green and a red signal light indicated "start" and "game over," respectively. Concealed in the upright portion of the apparatus was a panel of relay switches used to produce the same fixed sequence of 16 scores for each subject. The sequence of scores was 10, 5, 15, 10, 20, 5, 15, 10, 20, 5, 15, 20, 5, 20, 15, 10.

Additionally, a button-operated token dispenser which delivered a stainless steel coin the size of a penny replaced the large bowl of plastic tokens used in the earlier research.

*Procedure.* Each subject was brought individually from the classroom to a mobile research trailer located on the school grounds. While en route, the experimenter told the subject that she was from a toy company and that a new game was being tested to see how well people liked it. A male adult (the model) was waiting inside the trailer when the subject arrived, except when the subject was in the no-model group. The experimenter introduced him saying, "This is Mr. Hill. He's here to play the game with you." She briefly explained the operation of the game and demonstrated the use of the token dispenser. Then she explicitly instructed each child that tokens were to be taken while playing only for scores of 20 (the *stringent* rule) "because 20 is a good score and deserves a token." Furthermore, the subject was told that the tokens could be turned in for valuable prizes at the end of the game and that "the more tokens you have, the better your prize will be." The experimenter subsequently showed the child a varied display of school supplies and other prizes, ranging in value from $.05 to $10.00. At this point the experimenter left, and the model introduced the relevant experimental manipulations. The experimenter then returned to tell the child that he or she would be given the opportunity to play alone and exited.

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[3] Violations of stringent rules (in the direction of increased leniency) sometimes involve a hierarchy of deviant behavior and, of necessity, include behavior which complies with the stringent rule. Thus, in this experiment, adoption of the lenient rule of taking tokens for scores of 15 and 20 includes taking tokens for scores of 20, an action which is in partial accord with the stringent rule of self-administering tokens only for scores of 20.

All subjects received the same preset program of scores during their 16 trials. During the test period the behavior of each subject was recorded by a second experimental confederate who monitored the entire proceedings from the trailer's observation booth. Once the testing phase was completed, the observer signaled the experimenter to return. Thereupon, the experimenter asked the child if he had enjoyed the game, counted his tokens, and gave him one of the more useful displayed prizes (e.g., pencils, erasers, or notebooks).

*Deviant symbolic-modeling cues.* Half of the subjects were exposed to the deviant (i.e., *lenient*) standard through symbolic-modeling cues, and the rest were not. Deviant symbolic modeling referred to the model's statement to the subject that "I have just played the game, and I only took tokens for scores of 15 and for scores of 20."[4]

*Deviant live-modeling cues.* Half of the subjects in each of the aforementioned groups were exposed to deviant live-modeling cues, while the remaining subjects were not. In deviant live-modeling, the model played the game in the presence of the subject and took tokens only for scores of 15 and 20, accompanying his actions by appropriate verbal remarks: for example, "15, I'll take a token for a 15," or "5, I won't take a token for a 5." For subjects exposed to both symbolic and live modeling, the symbolic cues were presented first.

TABLE 1

ANALYSES OF VARIANCE OF SUBJECTS' SELF-ADMINISTRATION OF TOKENS FOR SCORES OF 15 AND 20: EXPERIMENT I

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Scores of 15: | | | |
| Symbolic model (A) | 1 | 9.375 | 5.282* |
| Live model (B) | 1 | 35.041 | 19.741** |
| $A \times B$ | 1 | 2.042 | 1.150 |
| Error | 20 | 1.775 | |
| Scores of 20: | | | |
| Symbolic model (A) | 1 | .167 | $<1.00$ |
| Live model (B) | 1 | 13.500 | 6.984* |
| $A \times B$ | 1 | .166 | $<1.00$ |
| Error | 20 | 1.933 | |

* $p < .05$.
** $p < .001$.

*Dependent measures.* A record was made of the trials on which token self-rewards were administered during the period in which the child performed alone. The data were collected, in the absence of both the experimenter and the model, by an experimental confederate who monitored the entire procedure from the trailer's one-way observation booth.

## *Results*

Inspection of the self-reward data for males and females separately revealed no tendency for sex differences; consequently, the data for both sexes were combined in all analyses. Furthermore, examination of the data indicated that more than 80% of the subjects did not take any tokens for scores of 5 and 10; therefore, the data were examined through separate two-way analyses of variance of subjects' self-administration of tokens for scores of 15 and for scores of 20.

Table 1 contains a summary of the analysis of variance performed on subjects' self-reward for scores of 15. As predicted, significant stringent rule violation was produced by observation of deviant live modeling and by exposure to deviant symbolic modeling. The interaction was not significant. Figure 1 presents the mean number of rewarded trials in each group, inspection of which reveals the hypothesized additivity with respect to the self-administration of rewards for scores of 15.

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[4] The behavior *reported* in the deviant symbolic-modeling condition was a summary description of what the child could *observe directly* in the deviant live-modeling condition (i.e., taking a token only when a score of 15 or 20 was obtained). Though the symbolic presentation involved a briefer exposure to the information than did its live counterpart, both reflected the same unit of play (i.e., *one game*). Furthermore, the naturalistic occurrence of symbolic cues frequently entails a shorter time span than that of the corresponding live cues. The two modeling treatments used in the experiment were therefore considered to be appropriate for the desired comparison, despite disparities in exposure time.

The probability of the means aligning in the predicted order by chance is 1/4! or $p < .05$.

A summary of the analysis for scores of 20 is presented in Table 1. Examination indicates that only exposure to a deviant live model had a significant influence. As seen in Figure 1, observation of a live model produced more self-administration of tokens for scores of 20 than did either exposure to a symbolic model only or to no model.

## EXPERIMENT II

In everyday life, occasions continually arise in which an individual observes two or more persons responding either similarly or differently to the same situation, a phenomenon referred to as *multiple modeling* (Bandura & Walters, 1963). Recent research indicates that rule adoption may be a joint function of direct instruction and multiple modeling in which the models employ divergent rules (e.g., Bandura, Grusec, & Menlove, 1967; McMains & Liebert, 1968). Thus, Experiment II was designed to replicate, in a multiple modeling situation, the finding of Experiment I that deviant live-modeling cues have a greater deleterious influence on rule adoption than do deviant symbolic-modeling cues. Live- and symbolic-modeling cues, presented by two models, were employed, and it was predicted that observation of a lenient live model would weaken stringent rule adoption more than would exposure to a lenient symbolic model, irrespective of the additional presence of a stringent live or symbolic model.

With respect to multiple-modeling cues, McMains and Liebert (1968) found that successive observation of a stringent model and a lenient model yielded differential amounts of stringent rule violation depending on the order of presentation; that is, there was an apparent recency effect. However, the investigators suggested that the finding might be an artifact arising from the dual role of their first model, who had also served as the training agent. Therefore, the present experiment had the additional purpose of reexamining order effects by presenting two models, neither of whom provided the direct instruction, in both lenient-stringent and stringent-lenient order.

The experiment employed a $2 \times 2 \times 2$ factorial design, the factors being: (*a*) order (Model 1, lenient–Model 2, stringent versus Model 1, stringent–Model 2, lenient), (*b*) type of modeling engaged in by Model 1 (live versus symbolic), and (*c*) type of modeling engaged in by Model 2 (live versus symbolic). In the presence of the models each subject was told the stringent rule by the experimenter. Subsequently, the subject was exposed either directly or indirectly to the self-reward behavior of the two different models, one stringent and the other lenient, thus making possible an examination of the hypothesis regarding the relative potency of live and symbolic deviant cues in a more complex situation than that used in Experiment I and including an examination of possible order effects. The design also permitted exploration of the relative influence of live and symbolic *nondeviant*-modeling cues.

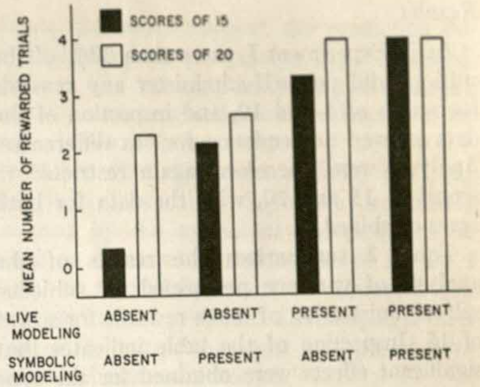

FIG. 1. Mean self-reward scores in Experiment I as a function of live and symbolic deviant cues by a single model.

### *Method*

The subjects were 16 boys and 16 girls selected from the second, third, and fourth grades of the same school that subjects in Experiment I attended. They were randomly assigned to the eight treatment groups. A female adult was the experimenter for all subjects, and two male adults served as the models. The apparatus was the same as that used in Experiment I, and the general procedure was also identical. Within any given group the two models alternated roles in order to counterbalance for possible experimenter effects. Model 2 observed as Model 1 displayed the relevant cues; following the departure of Model 1, Model 2 introduced the appropriate cues.

### *Results*

As in Experiment I, more than 80% of the subjects did not self-administer any rewards for scores of 5 and 10, and inspection of the data showed no tendency for sex differences. Analyses were, therefore, again restricted to scores of 15 and 20, with the data for both sexes combined.

Table 2 summarizes the results of the analysis of variance performed on subjects' self-administration of token rewards for scores of 15. Inspection of the table indicates that significant effects were obtained for the type of modeling engaged in by Model 1 and the type of modeling engaged in by Model 2. While order had no significant effect on subjects' adoption of the stringent rule, it did enter into significant interactions: namely, Order × Type of Modeling Engaged In by Model 1 and Order × Type of Modeling Engaged In by Model 2. None of the remaining interactions were significant.

Table 3 contains a summary of the mean number of rewarded trials with respect to scores of 15. Inspection of these means suggests that observation of a lenient live model was sufficient to produce considerable deviation from a stringent self-reward rule, regardless of the other treatment conditions. Exposure to a deviant symbolic model yielded less self-leniency. The results of a Newman-Keuls test performed on the means confirmed statistically that treatment groups with deviant live-modeling cues produced significantly more self-leniency than did those with deviant symbolic-modeling cues ($p < .05$), whereas there were no significant differences within each of these two major divisions.

TABLE 2

ANALYSES OF VARIANCE OF SUBJECTS' SELF-ADMINISTRATION OF TOKENS FOR SCORES OF 15 AND 20: EXPERIMENT II

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Scores of 15: | | | |
| Order (A) | 1 | 3.781 | 2.612 |
| Type of modeling engaged in by Model 1 (B) | 1 | 13.781 | 9.518** |
| Type of modeling engaged in by Model 2 (C) | 1 | 19.531 | 13.489*** |
| A × B | 1 | 19.531 | 13.489*** |
| A × C | 1 | 11.281 | 7.791* |
| B × C | 1 | .031 | <1.00 |
| A × B × C | 1 | 1.531 | 1.058 |
| Error | 24 | 1.448 | |
| Scores of 20: | | | |
| Order (A) | 1 | 0 | <1.00 |
| Type of modeling engaged in by Model 1 (B) | 1 | 15.125 | 13.698*** |
| Type of modeling engaged in by Model 2 (C) | 1 | 10.125 | 9.170** |
| A × B | 1 | 0 | <1.00 |
| A × B | 1 | .50 | <1.00 |
| B × C | 1 | 10.125 | 9.170** |
| A × B × C | 1 | .50 | <1.00 |
| Error | 24 | 1.1042 | |

* $p < .05$.
** $p < .01$.
*** $p < .005$.

TABLE 3

MEAN NUMBER OF SELF-REWARDED TRIALS IN EXPERIMENT II

| Type of modeling engaged in by Model 2 | Type of Modeling engaged in by Model 1 (lenient) | | Type of Modeling engaged in by Model 1 (stringent) | |
|---|---|---|---|---|
| | Live | Symbolic | Live | Symbolic |
| Scores of 15: | | | | |
| Live | 3.25[a] | .75 | 4.00[a] | 3.75[a] |
| Symbolic | 3.25[a] | 0 | .75 | 1.50 |
| Scores of 20: | | | | |
| Live | 4.00[b] | 4.00[b] | 4.00[b] | 3.50[b] |
| Symbolic | 4.00[b] | 1.25 | 4.00[b] | 1.75 |

Note.—If Model 1 is lenient, then Model 2 is stringent; if Model 1 is stringent, then Model 2 is lenient.
[a] Deviant live model observed.
[b] Live model observed.

A summary of the analysis performed on subjects' self-reward for scores of 20 is presented in Table 2. Significant influences were found for type of modeling engaged in by Model 1, type of modeling engaged in by Model 2, and the interaction between the two factors. Order did not have a significant effect, nor did any of the other interactions. A summary of the mean number of rewarded trials with respect to scores of 20 can be found in Table 3. Examination of the means suggests that observation of at least one live model, regardless of whether he was stringent or lenient, produced a greater willingness to self-administer rewards for 20s than did exposure to two symbolic models.

## DISCUSSION

The combined results of the experiments reveal a consistent difference in the relative effects of live and symbolic deviant-modeling cues and appear to provide promising support for the heuristic conceptualization presented initially. The view that individuals are increasingly likely to eschew a rule which they have been taught as the information value of deviant-modeling cues increases was supported by confirmation of several specific predictions derived from such a perspective. Further, the results provide strong corroboration for the more general hypothesis that the private adoption of social rules by an individual is a joint function of the direct instruction which he receives and the modeling cues to which he is subsequently exposed, and, moreover, they suggest that even symbolic deviant modeling is demonstrably pernicious when compared to a control group (Experiment I).

Recall that the value of the available information concerning rule violation of others, presumed to vary with the type of modeling, was hypothesized to be a crucial determinant of rule adoption by the observer. Both experiments reported here provide results which are consistent with the position. Thus, it was predicted that live-modeling cues, when deviant from one's previous training, would have a greater deleterious influence than would deviant symbolic-modeling cues because of the greater information value assumed to be inherent in the former. In Experiment I, as anticipated, after being instructed to use a stringent self-reward standard, subjects exposed to deviant (self-lenient) live-modeling cues were more self-lenient than subjects exposed to deviant symbolic-modeling cues. Further, in the multiple-modeling situation used in Experiment II, deviant live-modeling cues again resulted in greater stringent rule violation than did lenient symbolic-modeling cues, and subjects' deviations were not differentially affected by the type of stringent modeling (i.e., live or symbolic) to which they were exposed.

Based on the assumption that information value varies with the number, as well as the type, of deviant-modeling cues provided, it was expected that the presentation of multiple deviant cues would operate additively. In Experiment I, the absence of any interaction between live and symbolic cues (both of which had significant effects) and the appearance of the predicted ordinal relationship among the means support the prediction derived from such a perspective. As anticipated, subjects who were explicitly instructed in the use of a stringent self-reward standard without additional exposure to any deviant information virtually never violated the rule, despite the fact that they were presumably tempted by the availability of powerful incentives. Moreover, examination of Figure 1 indicates that violation of the stringent rule increased in the predicted direction as a function of the number, as well as type, of deviant-modeling cues provided.

Although no predictions were made concerning differential effects of live and symbolic stringent (nondeviant) modeling, the experimental results in this regard can also be understood in terms of the information-value formulation. Note that the previously discussed effects of deviant-modeling cues can be viewed as disinhibitory, in that they increased subjects' willingness to take tokens for scores other than 20. The present sample of children, unlike those used in previous studies (e.g., Liebert & Ora, 1968; McMains & Liebert, 1968), also appeared to require exposure to a disinhibitory influence before self-administering token rewards even for scores of 20. Thus, in both experiments, children rewarded themselves more often for scores of 20 if they had seen a live model do so (irrespective of whether he also rewarded himself for scores of 15, see Table 3) than if they had not. This result appears to be another instance of the greater power of live-modeling cues relative to symbolic cues for disinhibiting subjects, an outcome consistent with the present formulation. Given such results, it is surprising that live nondeviant-modeling cues were no better than symbolic nondeviant cues for inhibiting self-reward for scores of 15 (Experiment II). Taken together, the pattern of findings tentatively suggests that type of modeling is a critical variable only for inducing disinhibition, but not for strengthening a prohibition.

With respect to the conceptual basis for such studies, it should be noted that in both of the experiments reported here differential information value was *assumed* on the basis of rational considerations. Thus, while the results rather consistently appear to support the heuristic value of the concept for predictive purposes, they cannot provide empirical confirmation of its assumptions. Future research should independently assess subjects' perception of the value of available cues and, thus, attempt to further strengthen the position taken here and to elucidate and define more precisely the mechanisms which underlie the present findings. However, the overall results do provide strong support for the hypothesized influence of observationally gleaned information about the practices of others in the socialization process. In view of the frequent occurrence of both deviant- and nondeviant-modeling cues in naturalistic training situations, delineating the variables that predict their effectiveness seems to have considerable importance. More detailed examination of the relative potency of live and symbolic cues and determination of the effective limits of each appear to be exciting issues for future investigation.

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# SOCIAL LEARNING AND PIAGET'S COGNITIVE THEORY OF MORAL DEVELOPMENT

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The present study provides a replication, with extended analyses, of a previous study of moral judgment by Bandura and McDonald. In a pretest using Piaget-type pairs of moral judgment stories, 77 5- to 12-year-olds indicated which of two children in the stories was naughtier (names) and then justified their judgments (explanations). Thirty-two of these subjects, half with a majority of high level judgments and half with a majority of low, were exposed to an adult model responding at the opposite level. A posttest with six repeated items, six new items, and six somewhat unusual items was given either immediately or 2 weeks after the modeling ("conditioning") session. Bandura and McDonald's results were replicated with additional support, but it was found that the extent of the modeling effects depended upon a number of variables either singly or in interaction: subjects' pretest scores, the particular measure (names versus explanations), time between conditioning and posttest, type of item, and direction of conditioning (up or down). A theoretical analysis showed that neither the present study nor that of Bandura and McDonald could be used directly to affirm or deny Piaget's hypotheses. Most of the present results serve as a basis for more differentiated statements concerning the models' effects, but some of the findings raise questions which cannot yet be answered within the social learning framework.

According to social learning theory (cf. Bandura & Walters, 1963), the child's acquisition of adult moral standards is, to a considerable extent, a gradual process of imitating the observable values and behavior of others. Piaget (1932), on the other hand, has maintained that the development of the child's moral judgments goes through a necessary sequence of stages in which the child must resolve a moral dilemma by progressively constructing his own ethical position. These contrasting views were discussed by Bandura and McDonald (1963) in a report of a study which had two aims. Their primary goal was to demonstrate "that children's moral orientations can be altered and even reversed by the manipulation of response-reinforcement contingencies and by the provision of appropriate social models [p. 275]." Their second implicit aim was to question the adequacy of Piaget's theory by showing that "moral judgment responses are less age specific than implied by Piaget [p. 275]" and that Piaget's theory of demarcated sequential stages of moral development could not be substantiated empirically. In the article itself the authors were somewhat cautious about the extent to which their findings posed serious problems for Piaget's theory. However, a discussion of the study in a more general presentation of social learning theory (Bandura & Walters 1963) indicates that they considered the results to have important negative implications: "The *so-called developmental stages* were readily altered by the provision of adult models [p. 209, italics added]." "Experimental manipulation based on a social learning paradigm, revealed that the developmental sequence proposed by Piaget is by no means predetermined or invariant [p. 207]." Since Piaget's theory rests fundamentally on the notion of invariant sequence, Bandura and Walter's claim almost implies a refutation of Piaget's theory, at least in the area of moral development.

The present paper deals with both theoretical and empirical issues raised by the Bandura and McDonald experiment and by the way it was discussed. On the theoretical side we will argue that Bandura and McDonald's interpretation of Piaget is open to question, and that their data have limited relevance to Piaget's position. On the empirical side we will treat their study within the framework of social learning theory, replicating and extending the experiment in order to clarify their results and interpretations. After a brief description of Bandura and McDonald's experiment, some specific theoretical and empirical issues will be set forth in detail.

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### *Bandura and McDonald's Experiment*

A group of 12 pairs of stories was read to a large number of 5- to 11-year-old boys and girls. Each story was about a child whose actions result in undesirable consequences. In each pair of stories, one child was intentionally "doing something naughty," but the consequence was small (e.g., he was stealing a cookie and broke 1 cup), while the other child was either trying to be helpful or was involved in an accident but a large negative consequence resulted (in opening a door he knocked over a tray and 15 cups were broken). Half of the pairs of stories were taken with slight modification from Piaget (1948) and the others were new. On the basis of pretest performance, subjects were chosen who consistently responded with apparently lower level moral judgments (low subjects,[2] judging on the basis of larger negative consequences) or who consistently responded with apparently high level moral judgments (high subjects, judging on the basis of good intent). We refer to "apparent" levels of moral judgment because there is some question whether Piaget would agree with Bandura and McDonald's criteria.

Two weeks after the pretest, with a different experimenter, two-thirds of the high and low subjects were introduced to an adult who was also going to be tested on the stories. This time 24 pairs of stories were read alternately, 12 to the adult who acted as model and 12 to the child. The model's task was to respond at a level of moral judgment (name and explanation) opposite to that of the child's, so that high subjects were to be "conditioned" down while low subjects were to be conditioned up. The other third of the high and low subjects were verbally reinforced by the experimenter for responses opposite to their general moral orientation but no model was provided. After the experimental phase each child was taken to another room where still a third experimenter read 12 more pairs of stories, 6 of which were new and 6 of which had been given in the pretest.

In all trials, the subject was asked to name the child in the story who was naughtier (names) and to state the reason for his choice (explanations). Only the names data were reported on the grounds that there was a very high correlation between the level of names and explanations responses during the pretest phase.

The pretest results showed a gradual increase in the average number of high level moral responses as a function of age. Both high and low subjects exposed to models showed a substantial (and approximately equal) change in responses in the direction opposed to their initial moral judgment level. This change held up during the immediately subsequent posttest when the model was not present. Subjects without models showed no significant changes from pretest to treatment to posttest.

Bandura and McDonald (1963) concluded that the results "fail to substantiate Piaget's theory of demarcated sequential stages of moral development" and that the "utility of Piaget's stage theory of morality is further limited by the finding that children's judgmental responses are readily modifiable, particularly through the utilization of adult modeling cues [p. 280]." The authors pointed to the powerful influence of social models, but did not clearly indicate whether they assumed that social modeling is either a necessary or a sufficient condition for the acquisition and modification of moral judgment responses.

### *Theoretical Issues*

The theoretical issues raised by the Bandura and McDonald study center around the definition and measurement of stages. Bandura and McDonald interpreted Piaget's stage theory as implying that moral judgment stages are (1) age specific, and (2) clear-cut, and that they follow a (3) predetermined, (4) necessary sequence.

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[2] Bandura and McDonald use Piaget's terms "objective moral orientation" and "subjective moral orientation" to describe subjects at lower and higher developmental stages, respectively. We have found that most readers expect an "objective" stage to come after a subjective one. Since we will be discussing both ascending and descending changes in stage, it seems that clarity would be facilitated by relabeling the orientations low and high although this implies more of an absolute dichotomy than either Piaget or Bandura and McDonald intended.

1. *Age specific.* Piaget has never argued that his stages are age specific. The ages he mentions are those at which 75% of the subjects pass a given criterion. Data, then, are discussed in terms of numbers of subjects passing, not number of items passed. Obviously, some subjects acquire high level moral judgments before the given age and even more may do so afterwards. Bandura and McDonald's provision of a cross-sectional growth curve of average number of responses, in which the longitudinal discontinuities may cancel each other out, is simply not relevant either to Piaget's discussion of the ages at which stages emerge, or to his discussion of continuity and discontinuity in development (cf. Piaget, 1950).

2. *Clear-cut stages.* Bandura and McDonald's stories were pretested to remove any irrelevant cues which might lead subjects to judge the depicted actions of children in terms other than intentions or consequences. The interviews were constructed to pull strongly for a choice of either alternative both in names and explanations. Bandura and McDonald, then, have constructed a situation to elicit clear-cut responses, but Piaget specifically disclaimed the idea that any given child will consistently perform only at one clear-cut stage. Using the orally presented stories with children between 8 and 10, Piaget (1948) concluded,

> Though we could not point to any stages properly so called which followed one another in necessary order, we were able to define processes whose final terms were quite distinct from one another. These processes might mingle and overlap more or less in the life of each child, but they marked nevertheless the broad divisions of moral development [p. 175].

Piaget then went on to search for more clear-cut distinctions by observing the behavior of *children much younger* than the 8- to 10-year-olds he had interviewed. He found that in natural settings the younger children more consistently acted on the basis of the consequences of their own and others actions and disregarded the intention behind these actions. Also, Piaget was concerned with a number of aspects of moral judgments, not only the intention-consequences dimension.

3. *Predeterminism.* It is not clear what Bandura and McDonald meant when they said that Piaget's theory refers to a "predetermined" sequence of stages. In one statement they apparently minimized Piaget's emphasis on social interaction. "Typically the emergence of these presumably age specific modes of behavior is attributed to ontogenetic factors rather than to specific social stimulus events [p. 274]." In another paraphrase of Piaget's position, they stated, "As the child matures—he gains increasing autonomy, his relations become based upon mutual reciprocity and cooperation giving rise to the emergence of subjective morality [p. 275]." This latter statement correctly implies that Piaget considers social interaction to be an important factor in the change from lower to higher levels. He is not, then, positing a predetermined developmental force in the sense of a maturation theory like that of Gesell. However, for Piaget it is not the opportunity to copy models which is the important part of social interaction. Rather, it is the fact that interaction forces the adoption of more than one point of view. Thus, social interaction provides an impetus for disturbing present cognitive organization, leading to a state of disequilibration, and ultimately resulting in a new level of cognitive organization. This new level follows both logically and psychologically from the old and only in that sense is it predetermined.

One of the most cogent arguments against social imitation as *the* prime variable in the learning of moral judgments is the fact that lower-level judgments predominate at earlier ages. This predominance occurs in spite of the fact that adults presumably do not provide pervasive models of, or reinforcement for, lower level responses. If it is to be argued that adults do in fact model and reinforce low level judgments then it is difficult to explain the observed change in level of moral judgment with age; that is, social learning cannot have it both ways, at least not without a much more complex explanation of modeling effects.

4. *Necessary sequence of stages.* The crucial

question raised by Bandura and McDonald's experiment is whether they provide an adequate demonstration of a reversal in the "necessary sequence" of stages of moral development. We must also keep in mind that the possibility of reversing a sequence in a laboratory does not necessarily disprove the universality of the sequence under natural conditions of life. Bandura and McDonald's pretest data did support the idea that high level moral judgments are more often found in older children.

Piaget's general theory of development claims to describe changes in stages of cognitive organization. Social learning theory tends to be skeptical about the notion of stages, preferring to discuss or measure general classes of stimulus-related behavior. For Bandura and McDonald, the classification of a child as having a particular moral orientation is based upon the relative number of name responses which he gives at a low or high level. In their experiment the average number of high responses for the low subjects in the pretest was approximately 2–3 out of 12 (and vice versa for the high subjects). This means that most of their subjects were not pure cases; they tended to be in transition. Consequently, Piaget, as well as social learning theory, would expect these children to change in either direction as a result of environmental pressure.

Piaget makes a distinction between responses to tests and the general organization or structure of reasoning underlying the responses. The child's explanation of his moral judgment concerning who is naughtier is just as important as the choice itself. In Bandura and McDonald's study the data concerning explanations were obtained in all phases of the study but were only reported for the pretest. Despite the fact that a high correlation between names and explanations scores existed during this phase, it is possible that conditioning might lower this relationship in subsequent phases. A demonstration of change in necessary sequence of development in Piaget's terms, then, requires the use of explanations data throughout.

The manner of probing for explanations is also an issue in the diagnosis of stage. In Piaget's clinical method, resistance to adult countersuggestions is part of the technique used to assess the child's level; for example, after a child gives an answer, Piaget may say "Yes, but the child who was here before gave quite a different answer," and then go on to elaborate it. We are not presenting the circular argument that if modeling had an effect, the children were not really "at" a given level, and if it did not have an effect, then social learning theory is wrong. Rather, we wish to indicate that Piaget and social learning theory may sometimes be dealing with different phenomena, measured in quite different ways.

The above considerations indicate that Bandura and McDonald have not always stated Piaget's position accurately and have not provided definitive procedures to assess stages or changes in stages of moral development.[3] In the present study, we have accepted most of Bandura and McDonald's procedures in order to replicate and clarify their findings within their own framework whenever possible. Therefore, our experiment will also have limited applications to the moral stage phenomena discussed by Piaget.

In addition to the methodological issues, one aspect of Bandura and McDonald's empirical findings raises further questions concerning their claim to have altered the subject's developmental *stage*. The authors failed to comment on the finding that subjects changed from a 20% level of response in the pretest to approximately 50% in the conditioning and posttest periods (see our Figure 2 for the relevant data from their study). While this clearly represents a change in moral judgment responses, a 50–50 split does not clearly indicate that a new stage rather than confusion or conformity has been achieved.

### *Empirical Issues*

Although Bandura and McDonald's experiment has little direct bearing on Piaget's theory, it begins to study an interesting phenomenon. It goes beyond most social learning studies of the effects of models on the copying of simple behaviors, to an examination of social influence on the complex kinds of verbal judgments which children make. Unlike the usual experiment in which the subject is placed in an environment exactly like that of the model, the children had to make choices in stories with similar form but different content than that presented to the model. As the first study in this field, of course, it examined changes in a limited type of response, over a limited time period, under a restricted number of experimental conditions. The present study raises a number of questions concerning the effects of certain methodological variables, subject variables, and experimental conditions on the adult model's influence.

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[3] Admittedly, this is difficult to do. For the controversy in another area of Piaget's work see Braine (1959, 1964) and Smedslund (1963, 1965).

5. *Effects of subjects' pretest levels.* Bandura and McDonald's subjects *averaged* about 20% of their responses in the to-be-conditioned direction during the operant (pretest) phase. Since Piaget suggested that children in transition would be more susceptible to social influence, it is important to compare subjects close to extreme high or low responding with subjects who are closer to the middle of the continuum.

6. *Conditioning phase.* No trial-by-trial data were presented for the conditioning phase of Bandura and McDonald's study. The number of responses in the conditioned direction during the conditioning phase averaged 50%. The acquisition curves should be examined in detail in order to ascertain whether the curves are typical learning curves. If they rise gradually from 20% to about 80%, then the 50% posttest scores, obtained immediately after conditioning, actually represent a substantial decline in responses in the conditioned direction. This would not indicate that models are ineffective, but would serve to qualify statements concerning the extent to which the modeling effects were generalized.

7. *Names versus explanations scores.* In the theoretical section it was noted that only changes in "names" scores were examined. Explanations scores, in response to the question "Why was ______ naughtier?" do not fully correspond to Piaget's assessment of moral reasoning, but they do provide another index of level or moral judgment. Despite the high correlation of names and explanations scores in the pretest, we need to know whether explanation would be affected by adult models in the same way.

8. *Generality of change.* (*a*) Time between conditioning and posttest: Since all developmental theories accept the fact of short-term changes in response, it is incumbent upon Bandura and McDonald to demonstrate that their effects were long term. The posttest for generalization was given by different experimenter's than those used in the conditioning phase, but it was given in the same school immediately after the conditioning trials. It is important to ascertain whether the effects are maintained over time.

(*b*) New versus repeat items: The question of whether the changes in moral judgments were generalized was only partly answered by Bandura and McDonald's study. We know that both new and repeat items were given in that posttest but no separate analyses were presented. It is possible that children altered their responses to repeated items but not to new ones and thus arrived at the 50% posttest level.

(*c*) Stimulus cues: On what basis did subjects discriminate and then generalize the desired response? Bandura and McDonald assumed that subjects shifted from judgments based upon intent to judgments based upon consequences, or vice versa. However, as Turiel (1966) has pointed out, all Bandura-McDonald-Piaget items contrast good intentions and large negative consequences with bad intentions and small negative consequences. Even though the model gave an explanation along with each name choice, subjects could have been learning a discrimination based on amount of consequences. This possibility is investigated in the present study by adding some items in which good intentions were paired with small negative consequences and bad intentions with large ones. In this case, it is obvious which child in the stories is naughtier, but the subject is given a choice of explanations.

9. *Effects of direction of conditioning.* In Bandura and McDonald's study, the models' influence in conditioning subjects from low to high was approximately equal to the effect of conditioning from high to low. This result is consistent with the social learning position that moral judgment responses are habits

which can be modified in any developmental direction. Organismic-developmental theories such as Piaget's imply that it is possible to change responses, and possible but very difficult to alter the child's stage. If changes occur, they should take place more readily in the direction of spontaneous development than in the reverse direction.[4] In some studies using Kohlberg's (1964) technique for diagnosing stages of moral development, Turiel (1966, 1969) hypothesized and found that subjects exposed to moral judgments in a stage directly above their dominant stage changed in that direction more than did subjects exposed to examples two stages above or one stage below. The posttests were conducted 1 week after conditioning in contrast to Bandura and McDonald's immediate posttest.

### *The Present Study*

The experiment to be described below represents an attempt to replicate Bandura and McDonald's study as closely as possible, while at the same time adding some controls and some extended analyses in order to answer the empirical questions just raised. The study is not a hypothesis-testing experiment in the usual sense. It seeks to provide a more differentiated statement concerning the conditions under which, and the extent to which, adult models affect the responses of children to Piaget-type moral judgment stories.

## METHOD

### *Subjects*

Eighty subjects, 38 girls and 42 boys, were chosen randomly from the population of all children in one Berkeley elementary school situated in a low-income residential area. Their ages ranged from 5 years 6 months to 12 years 6 months. Since children were chosen in numbers proportional to the distribution of ages in the school, unequal numbers of subjects were tested at each age level. The data from 3 subjects were not complete; 77 subjects constituted the pool from which four matched subsamples were later drawn.

### *Experimenters*

The same two experimenters, one female and one male, served as testers through all phases of the study, and one female experimenter served as a model during the experimental or conditioning phase.[5]

### *Stimulus Items*

The present experiment employed 48 different pairs of stories. Before the pretest the experimenter clarified with the subject the meaning of "naughty." After each pair of stories was read, the experimenter asked "Which child do you think was naughtier?" (names) and then asked, "Why?" (explanations). Six of the items (i.e., pairs of stories) were Piaget's original set as modified slightly by Bandura and McDonald. They were used in the pretest and repeated in the posttest. Twenty-five items were taken directly from the Bandura and McDonald study,[6] with eight modified so that the material consequences in both stories involved the same kind of objects. Eleven new but similar items were created for reasons not pertinent to the present report.

It has been pointed out that all Bandura and McDonald items contrasted good intentions and large negative consequences with bad intentions and small negative consequences. Once having made the choice of which child was naughtier, one is almost committed to a certain level of explanation, that is, having said that John (who broke 15 cups) is naughtier, one must justify the choice on the basis of quantity of damage. In order to provide some alternative to this choice, six items were created in which good intentions were paired with small negative consequences and bad intentions with large ones. For example, the child helping his father accidentally spills a small amount of paint, while the child causing mischief dumps a whole bucket on purpose. These items were called "switch" items to distinguish them from both the new and repeat items described above. In these items all the children agree on which child was naughtier (names), but they have a choice in the explanation of their response. They can emphasize either the amount or the intent. These items were only used in the posttest in order to keep the first part of the experiment parallel with that of Bandura and McDonald.

Each item was separately scored for high or low level of moral judgment on the basis of names and explanations given by the subject. In the names scoring a high level of moral judgment was indicated by the subject's choice of the naughtier child as the one who showed "bad" intentions despite minimal consequences. This is a somewhat arbitrary procedure both because it ignores the child's possible confusion after having heard 9 or 10 pairs of stories and because, by adult standards, the designation of either child's intent as "bad" is often open to question. For example, it is not clear that a child who drops one of his sister's books in a puddle because she had been mean to him is a naughty child. However, if one is forced to choose the "naughtier" child, the alternative is generally clear. This raises Turiel's question (1966) of the extent to which some of these items actually represent *moral* judgments; at times the moral relevance is assumed by the experimenter rather than perceived by the child.

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[4] Spontaneous development is in the direction from low to high as both Piaget and Bandura and McDonald's Figure 1 suggest.

[5] We wish to thank Ed De Avila for serving as one of the experimenters and Carolyn Cowan for her participation as the model.

[6] Bandura graciously made available to us his items and his script for the model's responses.

In contrast to Bandura and McDonald's experiment where the experimenter questioned the child's explanations until a clear emphasis either on intent or consequences emerged, the experimenters in the present study tended to accept the child's explanation as long as it could be understood at all. This was decided during preliminary investigation in order to avoid forcing the child's answer into our preconceived categories and to avoid the social influence process during the pretest. Explanations ranged from clear emphasis on consequences (he was naughtier because he broke more cups) to justifications for the child's behavior (he did it because he was angry—an answer to why the child did it, not why it was naughty), to somewhat contradictory emphasis on both intent and consequences (she was naughtier because she helped her mother *and* the dishes broke), to clear-cut concentration on intent. Unfortunately the greater freedom allowed for subjects resulted in greater vagueness for scoring. After a number of unsuccessful attempts to arrive at interscorer consistency within a more refined scoring system, we had to resort to the original two-category high-low decision. A low response was one that clearly referred to the quantity of consequences as a subject's justification for his choice, while a high response was any other scorable response that focused on the motivation or underlying explanation for the behavior of the child in the story. The interrater agreement on explanation scores between two of the present authors was 89%. The interrater agreement between two new scorers was 88%. In neither case did the scorers have access to the name score when they were examining the explanations. Despite the fact that high-level explanations in this scoring system represented a more heterogeneous collection of responses than low-level explanations, the division between high and low appeared to be a meaningful one in Bandura and McDonald's framework. The correlation between level of names and explanations in pretest subjects chosen for further testing was .87 ($df = 29$).

We have here an illustration of good interrater agreement and high names-explanations correlation over all phases based on conceptually gross explanations data. The need to replicate Bandura and McDonald's procedure blocked the possibility that more detailed but open-ended questioning would have led to a more differentiated explanations scoring on which raters could agree. The possibility that the more differentiated explanation would show a lower correlation with names scores in conditioning and posttest phase will have to be examined in another study.

## *Procedure*

All 77 children responded to the 12 pretest items. As in the Bandura-McDonald experiment, groups for further testing were selected on the basis of their names scores—the number of times they had chosen the child with bad intent as naughtier rather than the child who caused the largest negative consequences. In order to make a clearer separation between subjects in transition and subjects at a low or high level, four groups of 8 subjects were chosen instead of two groups of 16. The groups were called definitely low (0–2 high responses) tentatively low (3–5), tentatively high (7–9), and definitely high (10–12). It should be noted that there were only 8 "pure cases," 5 subjects who responded to all 12 items at a high level and 3 who responded to them all at a low level. Because of our inability to complete the testing of one subject, there were only 7 subjects in the tentatively low subgroup. The groups were matched for age and were approximately equal in their distribution of boys and girls.

Two weeks after their pretest, subjects were each brought back to the experimental room. The experimenter explained that she wanted to find out what children and adults thought of the stories she was going to read, and so an adult (the model) would be working along with him. Twenty-four different item pairs were read, one to the model, and then one to the subject. The model responded according to the set of answers developed by Bandura and McDonald.[7] If the subject was in either of the low groups all the name choices and explanations of the model were at a high moral judgment level; the reverse was true for the high subjects. Both model and subject were given verbal approval for answers in the desired direction. Since Bandura and McDonald found that verbal approval accompanying modeling produced no greater effects than modeling alone, it was used here simply to make the experience more pleasant for the child.

In each group of subjects, half were given an immediate posttest after the model left the room. In order to examine the maintenance of effects over time the other four subjects were posttested after a 2-week interval. The posttest consisted of 18 items, the 6 Piaget items from the pretest (repeat), 6 "new" items, and 6 "switch" items (in which "bad" intent and large consequences were paired).

In summary, there were three phases of the study: pretest, experimental ("conditioning") and posttest. In each phase the child responded to 12 Bandura-McDonald-Piaget items. On the basis of their pretest scores, four groups of eight subjects were chosen, tentative and definite high and low. The low subjects were exposed to a model giving high moral judgment responses while the high subjects were exposed to a model giving low responses. Half of the subjects were posttested immediately after the experimental phase and half were retested 2 weeks later. In the posttest, six switch items were added to the six repeat items and the six new ones. The loss

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[7] If the subject were to be conditioned up, the model would explain her answer by contrasting the intent of the children in each story; if subjects were to be conditioned down, the model contrasted the amount of negative consequences without referring to the intent.

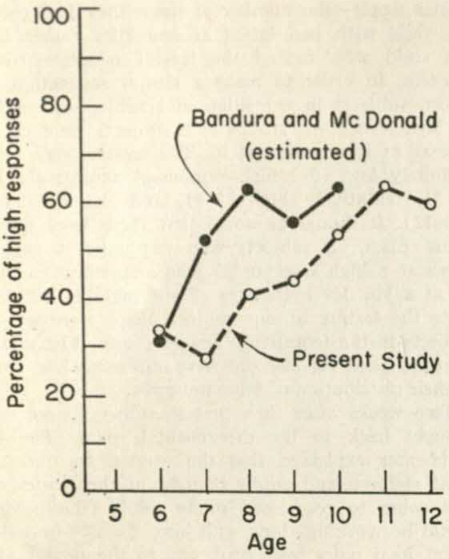

FIG. 1. Percentage of high moral judgment responses as a function of age.

of complete data from one up tentative subject left 31 experimental subjects in all.

## RESULTS [8]

*Replication.* It is necessary to establish that we have in fact replicated the Bandura and McDonald study. The percentage of high moral judgment responses (names) given by all 77 subjects is shown as a function of age in Figure 1. Also shown in this figure is an estimated average of male and female names scores derived from Bandura and McDonald's Figure 1 (p. 278). Within the age range common to the two studies (ages 5–10), the shape of the curves and the asymptotes were quite similar. The pretest data indicate that the developmental characteristics of the children in the two samples are comparable, although our sample shows a comparative developmental lag of 1–3 years at the lower age levels. It should be noted that Bandura and McDonald's subjects were largely upper-middle-class while the present subjects were lower-middle to lower-class.

Figures 2a and 2b present the average names and explanations scores in pretest, conditioning, and posttest with separate curves for subjects conditioned up and down; Figure 2a also includes the relevant points from Bandura and McDonald's Figures 2 and 3 (pp. 278, 280). In order to keep clear the between-study comparisons, data from the switch items in the present posttest were excluded.

It is evident from Figure 2a that the name score trends from pretest to the *immediate* posttest are remarkably similar to those reported by Bandura and McDonald. Starting from a pretest low of 20% of the responses in the conditioned direction, subjects conditioning scores moved to about 60% (about 10% higher than Bandura & McDonald) and remained close to that level in the immediate posttest. The results from Figures 1 and 2a indicate that we have satisfactorily replicated the findings of Bandura and McDonald. Given the present state of our field this represents a significant finding in itself. We can now proceed with a number of analyses enabling us to be more precise concerning the observed changes in subjects' behavior.

*Effect of subjects' pretest level.* A four-way analysis of variance performed on the name scores (up versus down; tentative versus definite; immediate versus 2 weeks; pretest versus conditioning versus posttest) indicates

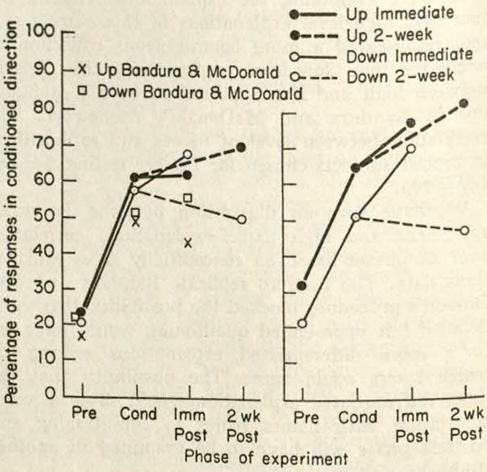

FIG. 2. Effects of a model on moral judgments as a function of phase of experiment, direction of conditioning, and time between conditioning and posttest.
a. (left) Names scores.
b. (right) Explanations scores.

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[8] Preliminary analysis revealed that there were no significant experimenter effects on pretest scores and so this possible source of variance was disregarded in any of the analyses described below.

that there were only two statistically significant effects, both main effects:

1. Conditioning and posttest name scores were significantly different from pretest scores ($F = 54.97$, $df = 2/46$, $p < .01$).

2. The difference between tentatives and definites, built in by selection in the choice of pretest groups, was maintained as a main effect during conditioning and posttest phases ($F = 11.72$, $df = 1/23$, $p < .01$). Tentative subjects, already farther in the conditioned direction than definite subjects during the pretest, finished the experiment closer to the experimenter's goal. The absence of interactions indicates that while the final level of responding was a function of the starting level, the *amount* of change shown by tentative and definite subjects was approximately equal and was not affected by the direction of conditioning. It will be seen below that the same results were present in the explanations scores.

The assumption had been made that a pretest score of 0–2 out of 12 (definites) indicated a stable low or high level, and that scores of 3–5 (tentatives) would designate subjects in transition. It is possible, however, that the specification of subjects definitely at a low or high level should have been more precise. In order to examine this possibility the average pretest to posttest change in names and explanations scores is shown in Table 1 as a function of pretest scores. For subjects classified as definites it is evident that the "pure cases" (subjects with no responses in the to-be-conditioned direction) changed less than subjects with a pretest score of 1 or 2. An interesting curvilinear relationship is present in both names and explanations scores; subjects with pretest scores closer to 5 out of 12 also changed less than subjects in the middle range. The greatest change in names scores occurred for subjects with a pretest score of 3, while the greatest change in explanations scores occurred for subjects with a pretest score of 1. Though it cannot be determined definitely from these data, it is possible that pure cases were more resistant to change, and that a ceiling effect reversed the change scores at the upper end. Whatever the interpretation, the equal amount of change shown by tentatives and definites is an artifact of the curvilinear relation between change and pretest level.

*Conditioning phase.* Before examining the explanations scores in Figure 2b we will take a closer look at subjects' performance during the conditioning phase. The learning curves are presented in Figures 3a (names) and 3b (explanations). The circles on the ordinate represent the average percentage of pretest responses in the to-be-conditioned moral judgment direction. The circles to the right of the learning curves represent percentage posttest judgments in the conditioned direction. In each graph the learning data are presented in blocks of two trials, and are separated for subjects conditioned up or down. Subjects conditioned up showed most of their learning of names responses within the first two trials, but gradually learned to match their explana-

TABLE 1

AVERAGE PRETEST TO POSTTEST CHANGE AS A FUNCTION OF PRETEST SCORE

| Scores | Pretest score | | | | | |
|---|---|---|---|---|---|---|
| | 0 | 1 | 2 | 3 | 4 | 5 |
| Names | | | | | | |
| *n* | 8 | 4 | 4 | 2 | 8 | 5 |
| *M* change | 4.75 | 6.50 | 6.50 | 8.50 | 3.75 | 4.00 |
| Explanations | | | | | | |
| $n^a$ | 4 | 4 | 6 | 7 | 3 | 2 |
| *M* change | 3.50 | 9.75 | 5.50 | 4.86 | 4.33 | 4.00 |

Note.—The amount of possible change ranges from 12, for pretest 0, to 7 for pretest 5.

[a] The *n* for explanations does not total 31 because 5 subjects began with more than 5 explanations scores in the to-be-conditioned direction. Their change scores are consistent with the above trends.

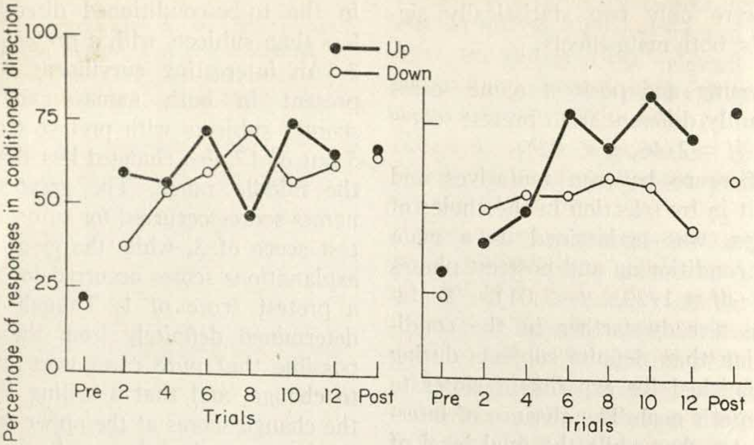

Fig. 3. Changes in moral judgment during conditioning. (The points to the left of the curves represent average pretest response and those to the right of the curves represent average posttest responses.)

a. (left) Name scores.

b. (right) Explanations scores.

tions responses to those of the model. By contrast, subjects conditioned down showed most of their learning of explanations responses within the first two trials, while gradually matching their name responses to those of the model. Responses in the posttest were equal to or above those at the end of the conditioning phase.

Figures 3a and 3b indicate that posttest responses did not represent a decline from conditioning performance. They also indicate either that the rate of learning names and explanations is different for subjects conditioned up and down or that different processes may be involved.

*Names versus explanations.* It should be recalled that explanations were scored as high or low on the basis of a rather arbitrary two-category classification. In the original attempts to develop a more differentiated set of categories, the disagreement among scorers increased as we moved from pretest to conditioning phase responses. However, when the two-choice category was used for explanations, scorer agreement did not vary across the phases. In increasing the scoring precision, it is evident that we may have lost important information concerning the precise nature of the changes in moral reasoning which could have occurred in this or in Bandura and McDonald's experiment.

The Pearson product-moment correlation between name scores and explanations was .86 in the pretest, .88 during conditioning, and .92 for the posttest phase. Despite the high correlation, the explanations data in Figure 3a and 3b revealed different trends than the names scores. Also, in Figure 2b, it can be seen that differences between groups seem more exaggerated than in Figure 2a. Subjects in each of the four major subgroups had been matched on the basis of names scores, but the pretest explanations scores for each subgroup need not have been equal. In fact (Figure 2b) the low subjects exposed to the up condition had begun by making more responses counter to their usual orientation than the high subjects exposed to conditioning down. This difference continued through the conditioning phase, but the posttest revealed a possible interaction between up-down and immediate–2-week variables in the explanations scores. Up and down subjects showed similar levels of response during the immediate posttest, both groups being farther in the conditioned direction than during the average of the conditioning phase. However, examining the subjects tested after a 2-week interval, we found that subjects conditioned up increased their high level while subjects conditioned down tended to remain at the conditioning phase level. This interaction in the explanations scores between up-down and immediate–2-week variables occurred only in the posttest and created a statistically significant triple interaction among direction of conditioning, time of posttest, and phase of experiment variables ($F = 3.87$, $df = 2/46$, $p = .05$). This effect is not present in the names scores. However, as in the previous four-way analysis of variance of names scores, the analysis of explanations shows that the tentatives moved significantly closer to the desired end of the scale than did definites, but there were no differences in amount of change. Again, Table 1 indicates this result to be an artifact of the curvilinear relation between pretest level and amount of change. No other main effects or interactions were statistically significant.

*Generalization effects.* The increased time between conditioning and posttest seemed to *facilitate* generalization of conditioning effects to posttest items, or at least did not interfere with the modeling effects (except for the down–2-week group's explanations scores). In order to determine whether this conclusion holds for all types of items, the differential effects of the main variables (up-down; tentative-definite; immediate–2 weeks) on repeat, new, and switch items in the posttest were investigated in a four-way analysis of variance. The analysis was performed on explanation scores because they alone provided a reasonable possibility of choosing either level of moral judgment in answering the switch items; that is, subjects could reasonably justify their name choice either in terms of intent or consequences in the stories. The statistically significant triple interaction ($F = 4.31$, $df = 2/46$) of direction of conditioning, time of posttest, and repeat, new, and switch items is shown in Table 2 by presenting the percentage of responses in the conditioned direction for each relevant cell. Also shown is the percentage of responses in the conditioned direction during the pretest and during the last four trials of the conditioning phase. Subjects conditioned up *kept going up in the posttest on all three types of items despite the absence of the model.* Subjects conditioned down and posttested immediately continued in the conditioned direction for repeat and new items, but showed less tendency to give the model's level of explanation in the switch items. After 2 weeks, high subjects conditioned down increased their modeling of repeat items over the last four trials of the conditioning phase. However, given a choice of explanation in the switch items, these subjects responded in the model's direction proportionately less often than they responded to regular items at the end of conditioning. In the new items they seemed to be returning toward their original high-level moral judgments.

TABLE 2

PERCENTAGE OF EXPLANATIONS IN CONDITIONED DIRECTION AS A FUNCTION OF DIRECTION OF CONDITIONING, TIME OF POSTTEST, TYPE OF ITEM, AND PHASE OF EXPERIMENT

| Phase of experiment | Direction of conditioning | | | |
|---|---|---|---|---|
| | Up | | Down | |
| | Immediate | 2 wk. | Immediate | 2 wk. |
| Pretest | 22 | 41 | 23 | 20 |
| Conditioning phase (last 4 trials) | 61 | 75 | 56 | 47 |
| Posttest | | | | |
| Repeat | 70 | 80 | 66 | 58 |
| New | 83 | 83 | 73 | 33 |
| Switch | 78 | 83 | 50 | 42 |

Since there were no switch items in the pretest, it is not possible to make conclusive inferences concerning *changes* in level of switch item responding. However, the lowered level of modeling effects in the down–2-week group seems to indicate a definite decline in modeling effects for these subjects. In light of the fact that responses to new items in the other groups stayed at the same level or at an increased level from the conditioning-phase, the score midway between conditioning and pretest level seems to represent a return to the original level of responding for the down–2-week group. This trend could be more firmly established with a longer conditioning-posttest interval.

## DISCUSSION

The two experiments under discussion were conducted by experimenters with different theoretical biases. The subject populations were different, some of the items varied slightly, and in our experiment, unlike Bandura and McDonald's, the same experimenter was present in all experimental phases. Despite these differences the basic results of the Bandura-McDonald experiment were replicated. Not only did models influence subjects' choices of which child was naughtier (names), but they also influenced the explanations or justifications given by subjects for their choices. Our study has established that children were not simply changing from one moral orientation to a state of random responding (the 50% conditioning and posttest level), and that they were not simply changing their responses on the basis of a discrimination of the amount of consequences in the stories (switch items). In general, then, the present results provide a great deal of support for Bandura and McDonald's contention that moral responses of children can be modified in either developmental direction by exposure to adult models. However, the central questions to be raised concern the precise specifications under which this modification occurs and the issue of what the modification may mean. The theoretical points raised in the introduction lead us to conclude that the findings do not necessarily have negative implications for Piaget's theory primarily because the diagnosis of stage and the opportunities for diagnosis of moral reasoning in both studies were limited to operations consistent with the social learning point of view. The present study essentially had to remain with Bandura and McDonald's procedures in order to demonstrate that their results had been replicated. Although the explanations data do provide results concerning an aspect of moral reasoning, Piaget's procedures would differ in the flexibility and scope of the interview and in the number of *dimensions* of moral judgment in addition to intention-consequences which would be assessed. We no longer have to assume that this "unstandardized" approach works against the discovery of lawful relationships, since Piaget's method has led to some of the most replicable results in the field of psychology. To focus more clearly on the issue of changes in stage, it would be helpful to replicate the format of the Bandura and McDonald study using Kohlberg's (1964) moral judgment stages, which are based upon Piaget's orientation and which are well worked out both theoretically and operationally.

The present study indicates that adult models have different effects under different methodological and experimental conditions. The pretest scores of the subjects, the measure of moral response (names versus explanations), the direction of conditioning, the time between conditioning and posttest, and the type of item (repeat, new, switch), singly or in combination, serve to moderate the model's influence. Under no combination of variables was the model completely ineffective, but the amount of conditioning and the maintenance and the generalization of changes were not uniform across all conditions.

There are three findings which necessitate further discussion: the complexity of conditioning phase trends, the fact that most groups *increased* their responses in the conditioned direction during the posttest, and the differences between subjects conditioned up and down. In the conditioning phase it is noteworthy that subjects conditioned up showed most of their learning of names in the first two trials, and that subjects conditioned down showed most of their explanations learning in the same period. Despite the fact that social learning theory acknowledges the possibility of one- or two-trial learning, it is still difficult to understand how a child's moral orientation could be changed by only two exposures to an adult model. We certainly need more information concerning what is happening during the time when the child is alternating responses with adult models. Social learning theorists tend to use the mechanisms of imitation as a prime explanatory concept in understanding changes in behavior. In the present situation it is still not clear what the subjects were imitating. They certainly could not copy the model directly, since they and the model always responded to items different in content. The switch items in the present study showed that subjects were not simply imitating the model's discriminations of amount of consequences. In order to substitute social learning principles of modification for Piaget's hypothesis of change through restructuring one's own activities and operations, it is as incumbent upon social learning theorists to clarify their use of basic terms as it is for Piagetians to clarify theirs.

There is a good possibility that the results reflect a social coercion or Asch effect rather than a change in moral reasoning. Piaget's theory of moral judgment suggests that the low child often tends to accept adult rules and statements as absolute, while the high child tends to make his own decisions. Thus, to the extent that social coercion can change a child's level of moral judgment, or at least his responses, the effect should be more long lasting in the low children than in the high (as we found). The interpretation that the child is temporarily responding to a form of social coercion is supported by the fact that high subjects maintained their new level of responding only on repeat items. Given new items (new or switch) after 2 weeks, these subjects either stayed at the conditioning level or, in the case of new items, began to revert to their pretest level of response. The details of conditioning effects could be examined in three ways. Another child employed as a confederate could ask the subject what "really went on" after the experiment. Second, there could be an experimental condition in which the subject is told to imitate the model in order to ascertain whether imitation could lead to even greater effects than the experimenters have obtained in the two studies under discussion. An unpublished study (Kuhn & Langer, 1968) indicates that the usual modeling experiment presents implicit instructions to the child to do what the model does. Finally, the tests for generalization could include moral judgments covering other moral dimensions and could take place outside the experimental setting in a real life context in order to obtain a better idea of the extent to which the model's influence can be generalized. If these tests should indicate that the present experimental paradigm elicits short-term social coercion effects, these effects would not be simple ones. The fact that up and down subjects showed different learning curves for names and explanations measures would also have to be explained.

In our experiment the conditioning effects were greater than those for Bandura and McDonald's subjects; some groups showed increased conditioning effects during the posttest. It is possible that even though the model was no longer present, the fact that the same experimenter gave the posttest was enough to reinstate the model's effects. This hypothesis does not, of course, explain why the responses of the down–2-week group declined.

The differences in performance of subjects conditioned up and down both in the conditioning and posttest phases is apparently not explainable within the point of view of social learning theory. If moral responses are simply habits, then there is no a priori reason why the model would produce somewhat different effects on low and high subjects. In the 2-week posttest, the modeling effects were much more pronounced on subjects conditioned up than on those conditioned down. Perhaps this is some indication that change is more likely to be maintained in the natural direction of development, as Turiel (1966, 1969) and Werner (1948) suggested. Perhaps, also, the experimental situation is perceived differently by subjects at different levels of development. Some of the further tests suggested above may help to clarify these interpretations. At any rate, it would be well for social learning theory to pay more attention to the developmental level of subjects as a moderator variable which may affect the influence of adult models on children's responses.

It is apparent that the results of Bandura and McDonald's study, which have been described as "unmistakably clear" (Zajonc, 1966, p. 49) are in fact exceedingly complex. While Piaget's point of view has been used in the present study to draw attention to some of this complexity, the theoretical differences between social learning and Piaget's cognitive theory of moral development remain unresolved.

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# SOCIAL LEARNING OF MORAL JUDGMENTS

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The present paper examines the theoretical implications of results based on a study by Cowan, Langer, Heavenrich, and Nathanson, demonstrating that modeling influences produce generalized and enduring changes in moral judgments. The limitations attributed to social learning theory for explaining these and other supplementary findings are questioned on both empirical and conceptual grounds.

Several years ago McDonald and I reported a laboratory study of moral judgments that had two major purposes. One of these was to demonstrate that a general principle exemplified in a variety of modeled responses can be transmitted on the basis of modeling processes. Previous investigations of modeling phenomena characteristically employed a restricted paradigm in which a model performs a specific set of responses and observers are subsequently tested for precise matching behavior in similar or identical situations. Under these limiting conditions, the experiments could yield only mimicry of specific modeled responses.

In an attempt to explore a higher order form of modeling, Bandura and McDonald (1963) utilized a paradigm in which a model exhibited a consistent judgmental orientation, but the model and the observers responded to different stimuli in the influence setting, and the occurrence of generalized modeling effects was measured in relation to new stimuli presented by different experimenters in a different setting with the model absent. To the extent that observers abstracted the judgmental principle from the discrete modeled examples, they could respond to new circumstances in a manner consistent with the model's disposition even though they had never witnessed the model's behavior in these particular situations.

A secondary purpose of the study was to test whether conceptual judgments that are assumed to reflect two successive stages of moral development in Piaget's theory could be significantly altered by social learning variables. The findings revealed that exposing children to adult models who expressed moral judgments that ran counter to the children's dominant evaluative orientations was effective in modifying their judgmental behavior in the direction of the social influence.

Cowan, Langer, Heavenrich, and Nathanson (1969) provided a partial replication of the foregoing study with some procedural variations to test further the generality and stability of the modeling influences. Consistent with results of the previous experiment, the authors found that modeling emerges as a significant determinant of moral judgments regardless of the direction in which judgmental behavior is being modified, the time at which the postexposure tests are administered, and whether the analysis is made in terms of the children's judgments or the reasons they give for their decisions. Moreover, modeling effects are evident even when the factors in the modeled examples that served as the major dimensions for making judgments are rearranged in new test items. This successful replication is all the more impressive, as the authors themselves acknowledge, considering that the two studies differ in the characteristics of the samples, in some of the experimental procedures employed, and in the theoretical commitments of the investigators. However, Cowan et al. concluded, somewhat surprisingly, that the findings are essentially irrelevant to Piaget's stage theory of moral judgment, and that the results pose interpretive problems for social learning theory.

The purpose of this invited commentary is to discuss briefly several issues raised by the authors, and to clarify areas of misunderstanding. The first of these concerns the nature of Piaget's developmental stages. The introductory section of the original paper outlined several general characteristics of stage theories, not all of which necessarily apply to Piaget's conceptual scheme. By juxtaposing statements taken from the general discussion and from a later description of the specific characteristics of Piaget's theory, Cowan et al. conveyed the impression of contradictions that do not, in fact, exist. According to Piaget's (1948) view, moral judgments follow a fixed developmental sequence. In the first stage, defined as objective responsibility, children judge the gravity of deviant actions in terms of material damages, whereas during the second, or subjective, responsibility stage, children judge conduct in terms of the intentions of the agent rather than the material consequences of the behavior. Piaget provided an approximate developmental timetable, although the transition from one stage to another is gradual rather than abrupt: "We have therefore two processes partially overlapping, but of which the second gradually succeeds in dominating the first [Piaget, 1948, p. 129]."

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The normative data based on the sample of children tested in our study reveal that objective and subjective judgments exist together at all age levels rather than forming successive stages that partially overlap. Cowan et al. dismissed these findings as irrelevant to Piaget's scheme on the grounds that children may achieve the developmental transition at different ages, with the result that longitudinal discontinuities may cancel each other out in a cross-sectional growth curve of average number of responses. It is surprising that the authors, who were able to find only five "pure" subjective cases and three "pure" objective children, were not dissuaded from this argument by their own data. Examination of individual records of the 154 children who were pretested in our (McDonald & Bandura, 1963) experiment similarly reveals that only 14% responded in a consistently objective or subjective manner, while 86% were in varying degrees of "transition." Both sets of empirical findings thus lend support to a conceptualization of judgmental behavior that assumes the existence of discriminative rather than uniform stage-determined responding.

A second issue concerns the relevance of findings derived from laboratory studies to Piaget's views about the modifiability of moral judgments and the processes whereby changes in conceptual judgments are achieved. Cowan and his associates assumed a somewhat inconsistent position on whether or not the judgmental performances measured in these studies are pertinent to Piaget's theory. Apparently, in an effort to minimize the significance of changes attained in children's moral judgments, the authors contended that since most of the children in the Bandura and McDonald study displayed a few responses counter to their dominant orientation, they were clearly in a transitional phase. One cannot argue on the one hand that a test provides an adequate index of developmental status but on the other hand that it is an inconsequential measure of developmental change. One might take issue with the arbitrary criterion of transitional status; nevertheless, the advocates of the criterion should apply it consistently in judging the two studies. Children in the Bandura and McDonald experiment who displayed 2 out of 12 responses counter to their dominant orientation were designated by Cowan et al. as in transition, but in their own experiment children who gave 2 divergent responses were classified as in a "definite" stage.

Before discussing the findings of the replication study, a correction should be made in the authors' description of the inquiry procedure utilized in the Bandura and McDonald study. After children expressed a judgment they were requested to provide an explanation for their choice and whatever reasons they gave were accepted. They were not questioned, as Cowan et al. reported, until they provided explanations specifically in terms of either consequences or the agent's intentions.

The authors questioned the comparability of findings from laboratory studies and from Piaget's investigations on the grounds that, in Piaget's clinical method of interrogation, after a child expresses his judgment he is informed that a preceding child responded in a different manner, the nature of which is then elaborated. The point is well taken. However, I would question the wisdom of employing such a procedure to assess treatment effects in view of evidence that being informed about the choice behavior of another person alters responses analogously to observing the same conflicting choices made by a real life model (Bandura & Mischel, 1965). Piaget's "countersuggestion" method represents more of a symbolic modeling treatment than an assessment procedure. This type of "countersuggestion" technique of testing would not only produce higher levels of change, but it would also confound the effects resulting from the experimental conditions themselves. In my judgment, a less coercive method for measuring changes increases rather than detracts from the value of findings.

Another issue in the replication study requiring comment concerns the data, their statistical evaluation, and the conclusions drawn from these findings. In accord with social learning theory, modeling influences produced significant changes both in the basis on which children judged the reprehensibility of the actions, and in the explanations supporting their judgments. Moreover, these induced changes either remained at the same level or increased slightly in the posttreatment assessments. The authors also reported three sets of supplementary findings that are difficult to evaluate because they are interpreted on the basis of visual inspection rather than statistical evaluation. Theoretical implications are drawn from small differences that, in many instances, may not be of statistically significant magnitude.

Children who differed in developmental level, for example, proved equally responsive to the modeling influence. The failure to obtain differential changes was attributed by the authors to an artifact of a curvilinear relationship between pretest level and amount of change. Considering the relatively small differences, particularly in the case of explanations, it is doubtful that a statistical test of the data presented in Table 1 would confirm this assumption. Similarly, in the absence of statistical evaluation of generalization effects across types of items (Table 2), the authors read more into the data than they probably yield.

Data are also presented for the frequency with which children expressed judgments and supporting explanations in the direction of the modeling influence across succeeding trials during the experimental phase of the study. The authors reported that the rate of learning of judgments and explanations is different for objective and subjective children. This may very well be true, but interpretation of these data should be held in abeyance until statistical tests are performed to determine which comparisons represent reliable differences and which are chance variations.

Intertrial comparisons are meaningful provided that individual items are equivalent. In the moral judgment task, items differ, among other ways, in the sex of the deviator, the gravity of the transgression, the nature and magnitude of the consequences, the characteristics of the injured party, and the circumstances under which the transgressive behavior is performed. The assumptions that these heterogeneous items are comparable is most likely untenable. Consequently, one must exercise considerable caution in interpreting not only variations in single responses in the experimental period, but also small differences in mean performances between treatment and follow-up phases, and between different types of items in the follow-up period. The problem of interpreting response changes does not arise in the Bandura and McDonald study because overlapping test items were used in the pretest and posttest phases of the experiment, and comparative data are available from a group of children whose judgmental behavior was not altered by reinforcement alone.

Had it been found that modeling outcomes were significantly influenced by all of the variables manipulated in the Cowan experiment, would these findings have posed interpretive problems for social learning theory? Contrary to statements by Cowan et al., a social learning approach does not assume that modeling outcomes are unaffected by the developmental status and other characteristics of observers. Indeed, a considerable body of research conducted within this conceptual framework (Bandura, 1968; Bandura & Walters, 1963; Flanders, 1968) reveals that observer attributes, either singly or in interaction with other variables, may partly determine the efficacy of modeling influences.

A social learning interpretation of human functioning further assumes that responsiveness, whether it be conceptual, social, or emotional, is partly under external stimulus control. Therefore, demonstration that performance of modeled responses may vary somewhat with changes in stimulus conditions would hardly constitute a controvertible finding.

Cowan and his colleagues also questioned whether social learning theory can explain the obtained changes in moral judgments because children could not copy the model directly. Such findings might present problems for nonmediational theories that depict modeling as a process involving exact response matching of a demonstrator's behavior. However, this is not the type of theory that I have been advocating. As explained elsewhere in detail (Bandura, 1968), in the author's formulation modeling stimuli convey information to observers about the characteristics of appropriate responses. In many instances, observers must abstract common attributes from diverse modeled responses and formulate a principle for generating similar patterns of behavior. The extent to which the resultant observational learning is activated into overt performance is governed by a variety of incentive variables and self-regulatory processes. Reliance upon symbolic functions is not a relevant dimension on which to differentiate between social learning theory and other conceptual formulations that invoke cognitive processes.

My concluding remarks are addressed to several additional misunderstandings regarding a social learning approach to developmental changes. Cowan and his associates reasoned that modeling cannot adequately account for the learning of moral judgments because it seems unlikely that parents evaluate deviant behavior in terms of consequences alone which predominate at earlier ages and, if they did, it would be difficult to explain how children later come to judge the gravity of actions primarily in terms of intentions. This argument contains several assumptions about parental behavior, about the agents of social influence, and about the consistency of moral judgments that are highly questionable from both an empirical and a social learning point of view.

Neither social learning theory, nor any other viable approach for that matter, assumes that parents behave in an invariant fashion toward their children regardless of the nature of their transgressions, the social or material consequences resulting from their behaviors, the circumstances under which they are performed, and the age of their children. Rather, parents generally behave in a discriminative manner so that under some circumstances they evaluate the reprehensibility of actions primarily in terms of their consequences, while under other conditions they may give priority to the offender's intentions. It would also come as no surprise to find that parents are more inclined to take intentions into account in judging their children's behavior as they advance in age.

A social learning position furthermore does not assume that parents serve as the exclusive source of moral judgments and behavior. Quite the contrary. The author's conceptualization of developmental processes (Bandura, 1968) calls special attention to the influential role played by extrafamilial adults, by peers, and by models presented in symbolic forms. Research guided by this point of view demonstrates that the influence exerted on children by exposure to adult standards of behavior is substantially altered by conflicting standards exemplified by peer models (Bandura, Grusec, & Menlove, 1967). To further complicate matters, modeling outcomes are significantly affected by dissimilarities in the response patterns displayed by different adults (Bandura, Ross, & Ross, 1963; McMains & Liebert, 1968), by inconsistencies in the behavior of the same model over time, and by discrepancies between the standards of behavior that a person models and those that he imposes upon others (Mischel & Liebert, 1966; Rosenhan, Frederick, & Burrowes, 1968).

Given existence of multiple, and often conflicting, modeling and reinforcement influences under naturally occurring conditions, it is not surprising that the vast majority of children exhibit varying admixtures of both objective and subjective moral judgments in their pretest conceptual behavior. Indeed, as shown earlier, children who display an exclusive subjective or objective morality are hard to come by at any age. One must, therefore, advance a theory that can account not only for generality in judgmental behavior, but also for specificity in evaluative behavior, which is the much more common phenomenon.

Results of the two experiments under discussion consistently demonstrate that moral judgments are more variable both within and between individuals and more modifiable than Piaget's theory would lead one to expect. Furthermore, modeling influences, which receive no mention in Piaget's account of the conditions regulating judgmental behavior, though they are obviously operative in everyday interactions, emerge as significant determinants. One might dismiss these laboratory findings as essentially irrelevant or as merely reflecting superficial conformity. An alternative response would be to acknowledge that modeling variables may function as potentially influential determinants of moral judgments that need to be verified by naturalistic studies. The former attitude insulates a theory from revision, whereas the latter judgment fosters research likely to advance understanding of basic processes governing the development and modification of moral judgments.

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# INFLUENCE OF SYMBOLICALLY MODELED INSTRUMENTAL AGGRESSION AND PAIN CUES ON AGGRESSIVE BEHAVIOR[1]

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This study investigated the independent and interactive effects of anger instigation, aggressive displays, and pain cues on subsequent interpersonal aggression. Adolescent delinquents were initially subjected either to anger-arousing or nonarousing experiences. They then viewed a nonaggressive control film or one of two films depicting a fight sequence: the pain-cues film, which focused on the victim's pain responses, or the instrumental aggression film, which highlighted the agent's aggressive behavior. The dependent measures were the duration and intensity of shocks that the subjects ostensibly administered to their provocateur whenever he made errors on an assigned learning task. The results revealed that (*a*) regardless of arousal level, subjects who witnessed the modeled aggression behaved more punitively than did subjects who had observed the same models behaving nonaggressively; (*b*) aroused viewers generally responded more punitively than did nonaroused viewers; (*c*) angered subjects who witnessed modeled pain reactions responded more punitively than did observers exposed to modeled instrumental aggression—for nonaroused subjects this difference was in the reverse direction; and (*d*) subjects with longer records of antisocial behavior delivered more aversive stimulation than subjects with less extensive records, particularly when they were angered and observed displays of pain. The overall findings contradict the catharsis hypothesis in both its classical and revised versions.

The catharsis hypothesis, whereby exposure to aggressive models reduces hostile impulses and consequently decreases the likelihood of future aggression, has been the subject of controversy at least since the time of the early Greeks. Whereas Aristotle contended that dramatic displays quieted the emotions, Plato maintained with equally strong conviction that they ". . . arouse violent emotions and stir men to all sorts of passions . . . [Jones, 1952, p. 250]." The concept of catharsis entered psychological theory by way of psychoanalysis (Hendrick, 1948) and eventually found its way into modern behavior theory in the classical monograph by Dollard and his collaborators at Yale (Dollard, Doob, Miller, Mowrer, & Sears, 1939). Only recently has systematic research (Bandura & Walters, 1963a; Berkowitz, 1962) necessitated substantial revision of this hypothesis. According to recent revisions by Buss (1961) and Feshbach (1961, 1964), the cathartic function of modeled aggressive stimuli is obtained only under certain specified conditions. Exposure to aggressive events is assumed to reduce subsequent aggression in hostility-aroused observers, but to augment aggression in nonaroused viewers.

A host of studies varying considerably in the form of modeled aggression and in the type of aggressive responses measured have found that exposure to aggressive stimuli under nonaroused conditions increases aggressive behavior in young children (Bandura, Ross, & Ross, 1961, 1963a; Hicks, 1965; Kuhn, Madsen, & Becker, 1967; Larder, 1962; Lovaas, 1961; Mussen & Rutherford, 1961), as well as in adolescents and adults (Walters & Thomas, 1963). Research evidence in support of the cathartic function of aggressive modeling cues under aroused conditions is meager and disputed on methodological grounds (Bandura & Walters, 1963a). While Feshbach (1961) has reported supportive results with college students, Berkowitz and his co-workers (e.g., Berkowitz, 1962, 1964a, 1964b, 1965; Berkowitz, Corwin, & Heironimus, 1963; Berkowitz & Rawlings, 1963) have reported increased aggression in angered subjects following exposure to aggressive models.

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[1] This study is partially based on a dissertation submitted to the faculty of the Graduate School of Stanford University, in partial fulfillment of the requirements for the PhD degree. An earlier abbreviated version of this paper was read at the 1967 meetings of the Western Psychological Association.

The author wishes to express his appreciation to Albert Bandura for his guidance throughout all phases of this research; to Keith Griffiths, Director of Research, California Youth Authority, for his assistance in acquiring subjects; and to David Geeting for his work in preparing the experimental films. This research was supported in part by Research Grant M-5162 from the National Institutes of Health to Albert Bandura.

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Although results of the above studies have shed some light on the determinants of the behavioral effects resulting from exposure to aggressive models, the different stimulus elements contained within an aggressive sequence must be isolated and their effects measured if the influence process is to be fully understood. An aggressive response sequence generally contains two separable and important stimulus events, that is, the instrumental aggressive response of the agent and the pain cues exhibited by the victim of the attack. Although a number of investigators (e.g., Berkowitz, 1964a, 1965; Berkowitz et al., 1963; Berkowitz & Green, 1966; Feshbach, 1961; Walters & Thomas, 1963) have reported changes in the viewer's aggressive behavior after viewing aggressive sequences containing both these stimulus events, no attempt was made to determine whether the depicted instrumental aggression or the pain cues are primarily responsible for the obtained changes. It is apparent from the work of Bandura and his collaborators (Bandura et al., 1961, 1963a, 1963b) that the observation of instrumental aggression in the absence of pain cues can result in the increased expression of aggression. The important question, then, is whether pain cues exhibited by the object of aggression augment (as suggested by the secondary-reward theory of Sears, Maccoby, & Levin, 1957) or counteract the disinhibiting and/or eliciting effects of the display of instrumental aggression. Pain cues might lower the likelihood of aggression by the observer through empathic or vicarious arousal of anxiety which would lead to aggression inhibition.

The present study was designed to test the revised form of the catharsis hypothesis, that anger arousal determines the function of aggressive stimuli, and to assess the effects of exposure to instrumental aggressive responses and pain reactions upon subsequent aggressive behavior.

To date most experimental investigations on the effect of film-mediated violence have used either young children or college students. Consequently, in view of the important social issue of the effects of televised violence on aggressive youngsters (Berkowitz, 1962; Klapper, 1960; United States Senate, 1964), and as a test of the generality of the previously obtained findings on film-mediated aggression, the present experiment was conducted with male adolescent delinquents.

The subjects participated with a peer who served as the experimenter's confederate in a study ostensibly concerned with the effectiveness of various instructional devices. In the first phase of the experiment anger was aroused in some of the subjects by having the fictitious confederate express derogatory remarks about the subjects' performance on an ego-involving task. Neutral remarks were made to the others. After this initial treatment, the subjects were assigned to one of three film treatments. Each film portrayed two adolescent boys playing basketball. The neutral control film depicted an active though nonaggressive basketball game. The two aggressive film displays portrayed an argument-fight sequence; the instrumental aggression film focused on the agent's aggressive responses, whereas the pain-cues film focused on the victim's pain reactions. Subjects were then provided with an opportunity to administer electric shocks to their anonymous provocateur, and the duration and intensity of electric shocks administered were measured.

Based on the theory advanced by Bandura and Walters (1963b) and Berkowitz (1962), it was predicted that subjects who had observed a film depicting instrumental aggressive responses would inflict shocks of greater magnitude and of longer duration upon the confederate than would subjects who viewed the neutral film. On the assumption that a person who serves as a source of anger will constitute a stronger stimulus for aggression than a person who has been associated with neutral experiences, it was also predicted that subjects who experienced prior anger arousal would likewise respond with more aggression than would subjects not so aroused.

Since there is no adequate a priori basis for estimating the effects of depicted pain cues, no predictions were advanced concerning the direction of the differences between subjects in the latter treatment group and those in the other film conditions.

## METHOD

### *Subjects*

Seventy-two male adolescents who were under court commitment to the California Youth Authority participated in this experiment. All subjects had been detained in the institution for a minimum period of 6 weeks and were awaiting either transfer to a treatment institution or home discharge They ranged in age from 13 to 16 years, with a median age of 15½ years. Of the subjects, 22% were institutionalized for highly aggressive offenses, for example, strong-armed robbery, assault, and fighting, while the remaining 78% had committed a wide variety of lesser crimes.

### *Procedure*

The subjects were individually contacted within the institution and asked to participate in an experiment on the effectiveness of teaching machines and audio-visual displays. They were then taken to a cable-strewn experimental room outfitted with a projector, screen, microphone, speaker, and the teaching machine which was a large impressive-looking apparatus covered with switches and various colored lights. The room and instructions were designed to lead the subjects to believe that the cable connectors, microphone, and speaker led into an adjoining room containing a peer who would be performing related tasks. The subjects were assured that the communication system and location of the participants in separate rooms would insure anonymity. This "fellow participant" was, in fact, a fictitious individual whose presumed responses to the experimental subject were recorded on tape in the voice of a delinquent boy from another institution.

After the subject had been given preliminary instruction on how he was to operate the intercom system to report his answers to the test items, the experimenter left the subject's room, ostensibly to give instructions to the fellow participant.

Aggression arousal was manipulated through the confederate's evaluative statements about the subject's performance on a nine-item multiple-choice test presumably measuring his ability to exercise good social judgment. One-half of the subjects underwent the aggression-arousal manipulation in which they were the recipients of a tape-recorded series of unwarranted and critical statements presumably from the confederate. The evaluator's tone was "cool" and disparaging as he challenged the subject's intelligence, "sophistication," and general competence. The remaining subjects assigned to the nonarousal condition received essentially neutral feedback comments.

Following the arousal manipulation, the subjects were randomly assigned to one of three film conditions. The 2-minute films were 16-millimeter black and white with sound tracks. The first minute of running time, which was identical for the three films, portrayed two boys shooting baskets on a basketball court. For the remaining minute the boys in the control film engaged in an active, though cooperative, basketball game, whereas in the two experimental films the boys engaged in an argument that eventuated in a fist fight. The pain-cues film focused almost exclusively on the victim's verbal and gestural pain reactions as he was vigorously pommeled and kicked by his opponent. The instrumental aggression film, on the other hand, focused on the aggressor's responses including foot thrusts, flying fists, aggressive verbalizations, and angry facial expressions. The subjects were told that the purpose of viewing the films was to determine how attentive people are to various film content. In order to enhance attending behavior and credibility, the subjects were further informed that they would be questioned about the film after the completion of the experiment.

After exposure, subjects were asked to assist the experimenter in a study ostensibly on the effects of performance feedback on learning rate. This aggression-performance task and accompanying shock-delivery system were patterned after the "aggression machine" and task originally designed by Buss (1961). The apparatus contained a panel with 10 switches arranged in increasing intensity together with lights that signaled right or wrong responses presumably made by the subject's partner in the adjacent room. After the subject sampled several of the shock intensities to apprise him of the magnitude of the aversive stimulation associated with the different switches, he was asked to administer the pain-producing shocks to his partner whenever he made an error on an assigned learning task. The subject was free, however, to vary both the intensity (which varied from slightly above threshold to quite painful) and duration of shocks administered. During the series of 25 learning trials, the subject's panel signaled 10 errors according to a prearranged program controlled by the experimenter. The duration and intensity of shocks inflicted upon the partner for his incorrect responses were recorded by the experimenter in an adjoining room through the use of a standard electrical timer and signal lights wired in series with the subject's shock-intensity switches.

Immediately following the measurement of aggression, the subject answered a series of questions, some of which were specific to the avowed purpose of testing the subject's memory for the film ma-

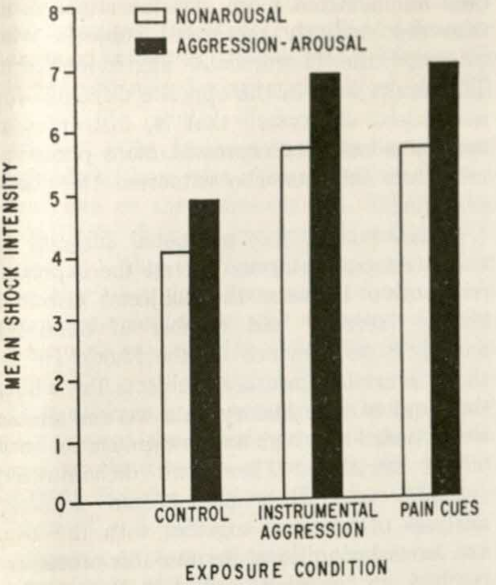

FIG. 1. Mean shock intensity for subjects viewing the control film, the instrumental aggression film, and the pain-cues film.

terial. Other questions attempted to elicit information concerning the credibility of the experiment and the participant's subjective response to the shock procedure. The relevant data will be described in detail in the Results section.

Before the subjects were returned to their living units, those individuals who had been criticized for their performance during the induction of aggression arousal were assured of the correctness of their responses on the test of social judgment. The experimenter also expressed amazement at the unreasonableness of their evaluator's comments.

## RESULTS

### *Effectiveness of the Arousal Manipulation*

One of the items in a questionnaire administered to the subjects at the conclusion of the experiment was designed to measure the effectiveness of the arousal manipulation; this item required the subjects to rate how well they liked their partners. It was expected that subjects in the aggression-arousal condition would express less favorable opinions of their partners than subjects in the nonarousal condition. Comparison of pairs of means between arousal conditions by the *t* test reveals significant differences in the predicted direction for each of the three film treatments ($p < .025$). Thus, the questionnaire data clearly indicate that the arousal manipulation was successful.

### *Measures of Punitiveness*

The primary dependent variables used in this experiment were the intensity and Intensity × Duration of shocks administered by the subjects. Separate analyses were made for each of these two measures.

*Intensity*. The mean shock intensities administered by subjects in the six experimental groups are presented in Figure 1. An analysis of variance performed on these data indicates that both arousal ($F = 8.67$, $df = 1/66$, $p < .01$) and the film treatments ($F = 11.04$, $df = 2/66$, $p < .001$) are highly significant sources of variance. Comparison of pairs of means by the *t* test indicated that as predicted, subjects who viewed the instrumental aggression film administered shocks of significantly greater intensity than did subjects who viewed the control film under both arousal conditions ($p < .01$). Exposure to the pain-cues film similarly resulted in significantly more intense shocks than did exposure to the control film, again under both arousal condi-

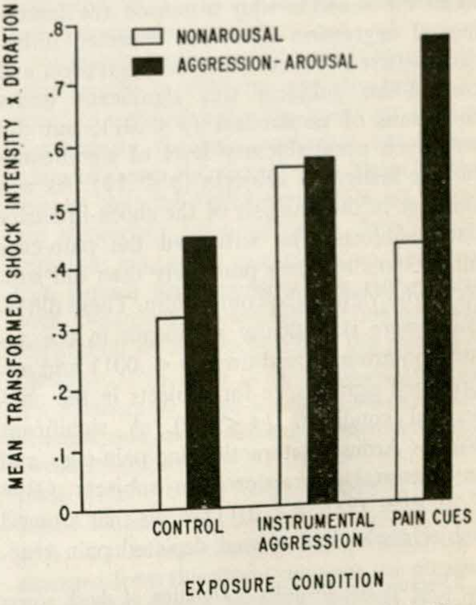

FIG. 2. Mean transformed Shock Intensity × Duration for subjects viewing the control film, the instrumental aggression film, and the pain-cues film.

tions ($p < .01$). No significant differences were found, however, between the pain-cues and instrumental aggression film subjects ($t < .50$). Also, as predicted, aggression arousal enhanced the intensity of aggressive behavior. This effect was significant in both experimental film conditions ($p < .05$) and, though not significant, was in the predicted direction for subjects who observed the control film ($p < .15$).

*Intensity × Duration.* The means of Intensity × Duration[2] of shock for the various groups are presented in Figure 2. An analysis of variance performed on these data discloses significant arousal ($F = 11.64$, $df = 1/66$, $p < .01$) and film ($F = 8.70$, $df = 2/66$, $p < .01$) effects and a significant interaction ($F = 4.69$, $df = 2/66$, $p < .05$). Selected $t$ test comparisons between pairs of means reveal differences similar to those found in the analysis of the duration measure. The predicted difference between the two arousal conditions was again significant, but only for the subjects who witnessed the victim's pain reactions ($p < .001$). This difference between arousal groups approached significance for the control film subjects ($p < .10$), but was extremely small for subjects who witnessed the instrumental aggression film. The expected difference between the instrumental aggression and control-film subjects was significant under conditions of nonarousal ($p < .01$), but did not reach a satisfactory level of significance for the instigated subjects ($p < .10$). As was the case in the analysis of the shock-intensity data, subjects who witnessed the pain-cues film responded more punitively than did subjects who viewed the control film. These differences were statistically significant in the aggression-arousal condition ($p < .001$) and approached significance for subjects in the nonarousal condition ($p < .10$). A significant Film × Arousal interaction for pain-cues and instrumental aggression film subjects ($F = 8.70$, $df = 1/66$, $p < .01$) reveals that aroused subjects who had viewed depicted pain reactions administered more aversive stimulation than did similarly motivated subjects who witnessed the instrumental aggression film. The results were in the opposite direction for nonaroused observers; that is, instrumental aggression subjects expressed more punitiveness than subjects who witnessed the pain-cues film.

*Offense record.* Two additional analyses of variance were performed to test the expected relationship between the subjects' criminal history (severity and number of offenses) and their performance on the Shock Intensity × Duration measure. Subjects for whom the required case history data were available were divided into high and low groups on both offense variables. These two dichotomized variables were then placed into separate analyses of variance together with the film and arousal conditions. Because this procedure resulted in unequal cell frequencies, unweighted-means analyses were performed.

The 57 subjects who had institutional ratings of aggressive behavior based on case-history data as well as offense record constituted the sample for the first analysis. Of this sample, 31 subjects had no history of violence, while 26 had a history of some or extensive violence. Analysis of these data indicates a slight trend for the subjects with a history of aggressive behavior to inflict more aversive stimulation ($F = 2.82$, $df = 1/44$, $p < .06$—one-tailed test). The interactions involving the aggressive-history variable were not significant ($F$s $< 1.75$).

For the second analysis the entire sample of 72 subjects was split at the median of the distribution of number of offenses and assigned to frequent and less frequent offender groups. Analysis of variance of these data reveals a significant main effect due to number of offenses ($F = 4.31$, $df = 1/60$, $p < .05$) and a significant Offense History × Film × Arousal interaction ($F = 5.04$, $df = 2/60$, $p < .01$). Neither the Film × Offense nor Arousal × Offense interaction was significant ($F$s $<$ 1.25). In order to illuminate the significant main and second-order interaction effects, means for the 12 Offense × Arousal × Film groups are presented in Table 1. As can be seen from this table, in four of the six comparisons between offender groups frequent offenders responded more punitively than less frequent offenders, though this difference was significant only for aroused subjects who observed the painful display ($t = 3.80$, $df = 60$, $p < .001$). In the case of the less frequent offenders, those who observed either the control film or the pain-cues film behaved less punitively than the comparable subjects who observed the instrumental aggression film, whether they had been aroused or not. By contrast, considering the frequent offenders who were angered, those who viewed the pain-cues film were substantially more aggressive than subjects in the remaining film conditions. To prevent the transformed scores from obscuring the meaning of the data, the mean of 1.0183 for the frequent offenders who were aroused and viewed the pain-cues film corresponds to a mean shock intensity of 8.23 (maximum of 10.0) for an average duration of 3.05 seconds!

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[2] Because the Intensity × Duration of shock scores were highly heterogeneous, all analyses involving these scores employed the $X' = 2 \text{ arc sine } \sqrt{X/100}$ transformation.

## DISCUSSION

The results of the present investigation provide further evidence of the influential role of vicarious experiences in modifying human behavior. More specifically, they demonstrate the degree to which the observation of symbolic aggressive models, and aggression arousal, can enhance punitive behavior.

The findings cast considerable doubt on the validity of the catharsis hypothesis either as proposed by Dollard and his collaborators or the more recent revision by Buss and Feshbach. According to the latter authors, exposure to aggressive models should decrease aggressiveness in viewers experiencing anger arousal. This hypothesis would also lead one to expect that subjects who have been instigated to aggression and shown the control film would display greater punitiveness than equally aroused subjects provided with an opportunity to view highly aggressive behavior. Not only do the results of this study fail to support the foregoing prediction, but they are also directly contrary to it. Indeed, under conditions of arousal, subjects who observed displays of instrumental aggression behaved more aggressively than did subjects who viewed the control film. Exposure of angered subjects to pain cues also augmented punitive behavior. This latter finding is essentially in accord with the theory proposed by Sears et al. (1957). According to this theory, if signs of pain and injury resulting from a child's aggressive behavior have repeatedly been associated with the elimination of frustration for himself, they will acquire secondary-reward value. Pain cues, therefore, should serve as positive reinforcers to enhance aggressiveness. Additional support for this theory has recently been provided by Patterson, Littman, and Bricker (1967), who found that pain cues exhibited by a victim increase aggression in nursery-school-age children.

TABLE 1

MEANS OF THE 12 OFFENSE FREQUENCY × AROUSAL × FILM GROUPS

| Condition | Film | | |
|---|---|---|---|
| | Control | Instrumental aggression | Pain cues |
| Aggression arousal | | | |
| Frequent offenders | .5020 (6) | .5307 (7) | 1.0183 (6) |
| Less frequent offenders | .3850 (6) | .6350 (5) | .5550 (6) |
| Nonarousal | | | |
| Frequent offenders | .3816 (5) | .6373 (6) | .3934 (5) |
| Less frequent offenders | .3277 (7) | .4873 (6) | .4499 (7) |

Note.—Cell *n*s in parentheses.

Although anger arousal generally increases punitiveness in subjects who have been exposed to aggressive models, it did not increase significantly the pain-producing behavior of subjects shown the control film. These findings suggest that the facilitative effects of arousal are dependent upon exposure to aggressive cues that may serve as eliciting or discriminative stimuli for aggressive behavior (Berkowitz, 1964a). The eliciting function of modeling stimuli as demonstrated in the present experiment with male delinquents corroborates the findings of other investigators with both younger and older subjects drawn from nondeviant samples (e.g., Bandura et al., 1961; Berkowitz, 1964a, 1964b; Berkowitz & Geen, 1966; Walters & Thomas, 1963).

A somewhat inconsistent finding that emerged from this study concerns the obtained interaction effects between arousal and the two experimental film conditions. Although no differences between the two aggressive-film groups were found on the intensity measure, the Intensity × Duration measure yielded a significant Film × Arousal interaction. A possible explanation for the differential results is that in the case of the intensity measure the subject deliberately selects a given shock intensity, and there is little ambiguity concerning his punitiveness. Duration of shock, on the other hand, represents a more ambiguous measure since the subject would find it more difficult to estimate accurately how long he kept the shock turned on. The lesser degree of cognitive control exercised with this measure may make it particularly sensitive to impulsive aggression following aggression arousal (Berkowitz & Buck, 1967).

It could be argued, however, that the combined measure which reflected both shock intensity and duration provides a more adequate measure of punitiveness, inasmuch as it includes the total amount of aversive stimulation administered. Justification for placing more confidence in the combined measure is provided by three subsidiary analyses. Correlations between mean scores on the two punitive measures indicate that both contribute unique variance to the Intensity × Duration composite. The average correlation for the six experimental groups equaled .44, with a range from −.25 for the nonaroused control film subjects to .80 for instigated subjects who viewed the pain-cues film. Second, intrasubjects analyses indicated that the two measures can, to a large degree, be viewed as compensatory; that is, 81% of the 68 subjects whose performance varied on both the duration and intensity measures over the 10 shock opportunities demonstrated a negative correlation between the indexes. Consequently, either measure taken by itself would not adequately reflect the amount of aggression displayed. A final attempt to evaluate the comparative worth of the aggressive-performance measures involved correlating them with institutional indexes of aggressiveness, that is, incidence of aggressive offenses and number of previous offenses. Although the correlations were generally low in magnitude and of borderline significance, correlations with the Intensity × Durations measure were generally highest in the three shock measures.

If confidence is placed in the Intensity × Duration measure, it would seem that when an individual is angered, particularly if he has a lengthy history of antisocial behavior, pain cues might serve as positively reinforcing stimuli (as suggested by Sears et al., 1957) and consequently augment aggressive behavior to an even greater extent than witnessing instrumental aggressive displays. This finding might also be interpreted as providing support for the hypothesis derived from the research of Bandura (1965) and his collaborators. According to Bandura's theory, the unpunished occurrence of aggression may not only convey permissiveness for aggressive actions, but it also may result in some degree of vicarious extinction of anxiety responses that are typically conditioned to intensive forms of physical aggression. Since punishment ordinarily follows an aggression-pain sequence with greater regularity than it does when aggression occurs without consequent injury to the recipient, it might be argued that the modeling of pain-producing aggression without any negatively reinforcing consequences to the agent may result in a greater degree of disinhibition.

In contrast, for individuals not aggressively aroused, exposure to instrumental aggressive responses produced more punitiveness than did exposure to modeled pain reactions. There was not a significant difference between the nonaroused subjects who observed pain reactions and those who viewed the neutral film. The former difference cannot be attributable to differences in implied permissiveness for aggression between the pain-cues and instrumental aggression films. Instead, it would seem that intense pain reactions, under conditions of nonarousal, produce an empathic response on the part of the viewers which results in aggression inhibition. Anger, therefore, would seem to raise the threshold for developing empathic inhibitory responses. Support for this interpretation is provided in a recent study by Feshbach, Stiles, and Bitter (1967). These authors found that shock to another person augmented pain-producing responses for insulted subjects; for noninsulted subjects the observation of pain cues resulted in a reduction in aggressive responding. Similar reductions in aggressive responding by nonaroused subjects following exposure to pain cues have also been reported by Buss (1966a, 1966b).

Based on the above reasoning, one might expect that increasing the intensity of depicted pain reactions would eventually produce a decrease in subsequent aggression; that is, the hypothesized relationship between pain cues and aggression is approximated by an inverted-U-shaped curve. Furthermore, it might be predicted that increased arousal would displace the gradient upward and toward the right, as would decreased socialization of aggression. Partial support for the hypothesized effect of decreased socialization of aggression can be found in the greater aggressive responses, particularly after observation of the pain-cues film, of aroused subjects with lengthy histories of antisocial behavior as compared with similarly aroused subjects with briefer criminal records. Further investigation of these hypotheses, as well as a functional analysis of pain cues for children of various ages, is currently in progress.

Although the specific relationship between the two types of aggressive cues and arousal must remain somewhat speculative as a result of this experiment, there is no doubt that a presumed "cathartic" experience did indeed enhance aggressive behavior.

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# ACTIVATION OF CUE-ELICITED AGGRESSION BY GENERAL AROUSAL

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Forty-eight male subjects witnessed either an aggressive boxing film or a film depicting nonaggressive sporting activities. All subjects then delivered electric shocks to an experimental accomplice. During such time subjects either were stimulated with white noise or were not so stimulated. The greatest degree of aggression toward the accomplice was manifested by subjects who had seen the boxing film and had also received noise. The results are discussed in terms of Berkowitz's proposal that arousal increases the probability that a person will react aggressively to aggressive stimuli.

In recent years Berkowitz and his colleagues have published a number of studies showing that subjects who are attacked and then exposed to stimuli associated with aggression later aggress against a victim more than subjects who have not been attacked (Berkowitz, 1965; Berkowitz & Geen, 1966, 1967; Berkowitz & LePage, 1967). Two other experiments have provided evidence that frustration created through a failure at a task can also facilitate the effects of aggression-associated stimuli (Geen, 1968; Geen & Berkowitz, 1967). In the latter studies, subjects who experienced task failure and then saw a violent film attacked another person (even though the victim was not responsible for the task frustration) more than did subjects who had succeeded at the task.

Berkowitz (1969) has interpreted such findings in terms of a revised frustration-aggression hypothesis by proposing that prior attack or frustration presumably produces general arousal, which, in turn, increases the probability of occurrence of aggressive responses to the cues present in witnessed aggression. Such an argument is consistent with a considerable body of theory and research suggesting that generalized drive facilitates the expression of responses which are dominant in a given situation (Hull, 1952; Spence, 1956; Walters & Parke, 1964; Zajonc, 1965). In the present context it is assumed that aggressive responses are dominant in a subject's repertoire after he has observed aggression and that general arousal or generalized drive then increases the likelihood that the aggressive responses will actually be made.

If undifferentiated arousal is an important consequence of many of the operations (such as interpersonal attack) which have been used to manipulate aggressive dispositions, and if such arousal interacts with environmental cues to produce aggression, then a similar aggression-facilitating effect may be obtained when the same aggression cues are paired with arousal from a different source. Any strong stimulus which is capable of arousing a subject can therefore be thought of as a possible antecedent of aggression; it need not necessarily arise from emotional situations such as task failure or interpersonal hostility. The investigators specifically propose that general arousal, as defined in the present context, is comparable to what has otherwise been called "activation" (Lindsley, 1951; Malmo, 1958, 1959). According to activation theory, input in any sensory modality has nonspecific effects which may manifest themselves in behavior as a general state of arousal. Reasoning from this activation notion, the investigators predicted that any sensory input can possibly create a sufficiently high level of arousal to activate or "energize" latent responses to aggressive cues. For purposes of the present study, the Hull-Spence approach to behavioral arousal is not distinguished from the Malmo viewpoint. While Malmo has emphasized the underlying physiological correlates of activation and followers of the Hullian theory have not, both positions postulate an energizing state of the organism which increases the probability of behavior. Such an energizing state is referred to in the present study as general arousal.

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In the present experiment, white noise was used as the arousing stimulus. Subjects first witnessed either a film portraying a violent scene or one showing a series of exciting but nonaggressive events. Subjects were then given an opportunity to aggress against another person. During the phase of the experiment in which they were allowed to aggress, half of the subjects were stimulated by white noise and half were not. The greatest amount of aggression should be observed in subjects who had heard noise after encountering the aggressive stimuli by seeing the violent film.

## METHOD

### *Subjects*

The subjects were 48 male college students who participated in order to fulfill part of a requirement for an introductory psychology course.

### *Apparatus*

Aggression was measured by an aggression machine adapted from that used first by Buss (1961). Noise was transcribed to a tape recording from a standard white noise generator and was presented to the subjects from the tape recording.

### *Procedure*

Arriving at the laboratory, the subject was met by the experimenter and by an experimental confederate posing as another subject. The experimenter informed both men that the study involved electric shock and gave the subject a chance not to participate if he so desired. All subjects expressed willingness to participate. The two men were then told that they would participate separately in different experiments: one would be in a test of the effects of stress on problem solving, and the other would be in an investigation of effects of distraction on memory. Subjects in the latter group would presumably be quizzed on the content of a movie. The experimenter then assigned the confederate to the "stress experiment" and the subject to the "distraction experiment."

The experimenter then explained the two experiments. The confederate was told that he would have 5 minutes to work on a problem before submitting a written solution to the subject for evaluation. The subject would meanwhile watch a short film, then evaluate the confederate's work, and finally take a short test on the content of the film. The experimenter instructed the subject to communicate his judgment of the confederate's answer by giving him from 1 to 10 electric shocks. The shocks were to be given at the subject's discretion, with 1 shock indicating to the confederate that his work was of good quality and 10 shocks indicating that it was extremely poor. The experimenter also informed the subject that he could vary the intensity of the shock by choosing from among 10 shock intensities, represented by the 10 buttons on the aggression machine. The purposes of the evaluation procedure were ostensibly both to place the other person (the confederate) under stress as he worked on the problem and to distract the subject from the film he had just seen.

The experimenter, after providing the previous information, seated the two men on opposite sides of a room divider and pretended to attach the shock bracelet to the confederate's arm while outlining the problem—to devise a plan whereby a department store could improve labor-management relations. The experimenter then returned to the subject and showed him a film. Half of the subjects (aggressive film condition) watched a 7-minute excerpt of a prizefight from the film, *Champion*. The experimenter introduced the movie with a synopsis of the story designed to reduce the subject's inhibitions against aggression (cf. Berkowitz & Geen, 1966). The other half of the subjects (control film condition) saw an equally long movie containing baseball, tennis, and weightlifting action.

After the film, the experimenter gave the confederate's written solution (actually a standard one used with all subjects) to the subject. Before allowing the subject to deliver shocks to the accomplice, the experimenter gave the subject a brief questionnaire, the main purpose of which was to measure the subject's level of self-reported anger. Immediately before filling out the questionnaire, half of the subjects (arousal condition) were informed that because of noisy interference from outside the laboratory they would be given a masking noise by means of earphones. These subjects then heard tape-recorded white noise at an intensity of approximately 60 decibels, a level determined by pretesting to be arousing, but not aversive.[2] The other half of the subjects (no arousal condition) were also instructed to wear earphones in order not to be bothered by outside interference, but were not stimulated by white noise. In both conditions subjects wore the earphones for approximately 2 minutes, after which the experiment ended. This was judged to be a short enough period of stimulation that no appreciable degree of habituation would set in for the aroused subjects.

The experimenter completed the experiment by informing subjects of the true nature of the manipula-

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[2] Because it has been shown that aversive stimulation can of itself produce aggression (Ulrich, 1966), the noise used in the present study had to be arousing but not noxious. A modified method of limits was therefore used by which seven subjects reported the intensity level at which white noise was aversive to them. The intensity level chosen for use in the main study was one below the reported noxious level; subjects reported this intensity to be stimulating but not aversive.

TABLE 1

Mean Number of Shocks Given by Subject

| Arousal treatment | Film condition | |
|---|---|---|
| | Aggressive | Control |
| Arousal | $4.58_{a}$ | $3.50_{ab}$ |
| No arousal | $3.16_{ab}$ | $2.91_{b}$ |

Note.—Cells having common subscripts are not significantly different at the .05 level by a Duncan multiple-range test.

tions. No subject expressed awareness of the experimental deceptions.

## Results

### *Shock Number*

The mean numbers of shocks given by subjects in the four treatment groups appear in Table 1. An analysis of variance for the results yielded a significant main effect for arousal ($F = 4.46$, $df = 1/44$, $p < .05$) and a slight but nonsignificant interaction ($F = 1.98$, $df = 1/44$, $p < .20$). The greatest number of shocks was given by subjects who received both the arousing noise and the aggressive cues, while the least aggressive subjects were those who received neither treatment. Most of the variance causing the main effect is due to the difference between such extreme conditions, as the result of the multiple-range test indicates. The fact that subjects who received both arousing noise and aggressive cues were the most aggressive of all subjects gives some support to the authors' prediction. It should also be noted that the greatest difference in aggression due to presence or absence of aggressive cues occurs among the aroused subjects.

### *Shock Intensity*

The intensity of shock given by subjects was taken as the number of the button on the aggression machine chosen for any given shock. The mean intensities chosen in the four conditions are shown in Table 2. The analysis of variance revealed no differences among the groups although the interaction effect barely failed to reach significance ($F = 3.50$, $df = 1/44$, $p < .07$). The results therefore provide a fairly strong suggestion of the predicted interactive effect. As in the case of shock number, the subjects who gave the most intense shocks were those who were presented with aggressive cues just prior to being aroused.

TABLE 2

Mean Intensity of Shocks Given by Subject

| Arousal treatment | Film condition | |
|---|---|---|
| | Aggressive | Control |
| Arousal | 4.56 | 3.43 |
| No arousal | 3.71 | 4.29 |

### *Total Shock*

A third measure was calculated, which was regarded as an indicator of the total amount of noxious stimulation the subject chose to give to the confederate. The measure represents a summation of intensities chosen by the subject across the number of shocks he gave.[3] Results based on this measure appear in Table 3. The main effect for arousal approached significance ($F = 2.86$, $df = 1/44$, $p < .10$) and was due to the high degree of aggression manifested by subjects in the arousal-aggressive film condition. The interaction effect was significant ($F = 5.64$, $df =$

TABLE 3

Total Shock Given by Subject

| Arousal treatment | Film condition | |
|---|---|---|
| | Aggressive | Control |
| Arousal | $22.25_{a}$ | $10.33_{b}$ |
| No arousal | $12.75_{b}$ | $14.75_{ab}$ |

Note.—Cells having common subscripts are not significantly different at the .05 level by a Duncan multiple-range test.

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[3] Shock number and shock intensity were not highly correlated in the present study. The coefficients in the four treatment groups were: arousal-agressive film, $r = .43$; arousal-control film, $r = .05$; no-arousal-aggressive film, $r = .09$; no-arousal-control film, $r = .46$. This finding is consistent with a similar one of no strong correlation between the two variables by Geen, Rakosky, and O'Neal (1968). Apparently intensity and number are not redundant; the subject tends to choose either a large number of shocks or extreme shocks in order to manifest aggressiveness. The total shock measure therefore indicates the operation of both processes within a treatment group.

1/44, $p < .05$), thus supporting the main prediction of the experiment.

### *Anger Ratings*

Analysis of subjects' self-reported anger showed no results which even approached significance. The finding suggests that the attempt at controlling for noxiousness of the white noise was successful. Conceivably, if the noise had been aversive to the subject, he would have reported himself as more angry than would subjects who heard no noise. The effects of the arousing treatment shown in the present study apparently can not be attributed to an intervening emotional variable such as anger, but to the properties of general arousal.

## DISCUSSION

The results of the present experiment support the hypothesis that general arousal and aggressive cues interact to elicit aggressive responses. The findings are thus consistent with Berkowitz's (1969) argument that general arousal facilitates the expression of aggressive responses to aggressive cues. Such observations are not meant to suggest that the point of view expressed here is sufficient to account for all aggression. Recent evidence from animal studies suggests that aggression may not be a unitary concept and that several different classes of behavior hitherto classified only as "aggression" in fact seem to follow from quite different neurological and endocrine antecedents (e.g., Moyer, 1967). Undoubtedly, much aggressive behavior represents a specific response to a specific class of stimuli. It is proposed, however, that much of the sort of aggression predicted by the frustration-aggression hypothesis may occur as a result of the effects of frustration-produced arousal upon aggressive responses which are dominant in a given situation. Such responses may be dominant either because of a high degree of aggressiveness habit strength in the person or, as the present study shows, because external aggressive cues have elicited the aggressive response.

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# AWARENESS OF OWN ANGER LEVEL AND SUBSEQUENT AGGRESSION[1]

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As a first test of the thesis that the individual comes to want to attack the person he believes has caused his anger reactions (internal visceral, motor, and ideational responses he labels as "anger"), 48 male university students were first exposed to an obnoxious accomplice of the experimenter and then were induced to think that they were either low, moderate, or high in anger toward this person. When given an opportunity to shock the confederate, the medium-anger subjects were significantly more aggressive than either the low-or high-anger men. Evidence is provided indicating that the high-anger subjects had inhibited strong aggressive responses because the knowledge that they were very angry had made them highly anxious, since the moderate provocation did not justify their extreme emotional reaction. The findings are discussed in connection with Schachter's theory of emotions, self-consistency theorizing, and the James-Lange theory of emotions.

How we react to an emotionally arousing event depends, at least in part, on our understanding of the situation and also on our interpretation of what is going on inside us. Obviously, we may inhibit some behaviors because we think they are dangerous or improper while giving free rein to other actions because they seem safe and/or permissible. More than this, however, our beliefs about the environment often determine also the specific nature of our emotional feelings and behaviors. Lazarus and his colleagues (cf. Lazarus, 1966) have demonstrated, for example, that an observer's interpretation of a filmed primitive circumcision rite affects the magnitude of the discomfort he will feel on seeing the movie and even the degree of physiological arousal created within him.

Going even further, Schachter (1964) maintained that differentiated emotional feelings and actions result from the individual's interpretation of his own internal reactions. Autonomic changes do not in themselves give rise to specific emotional behaviors and moods, Schachter argued. Exactly what feelings the person will have and what actions he will take upon being aroused—fear or anger, flight or fight—presumably depends on his understanding of the reactions within him. The subjects in Schachter's well-known experiment (Schachter, 1964; Schachter & Singer, 1962) were first aroused physiologically and then watched another person act in either a manic or aggressive fashion. Not knowing initially how to interpret their own feelings, they presumably used the other individual's behavior as a clue to what they themselves were feeling and then behaved in accord with this cognition.

Other writers have advanced a somewhat different view of the Schachter results. Also emphasizing the importance of the person's understanding of his emotional state, they have suggested that cognitions may affect emotional behavior after the initial reactions have taken place. Thus, Walters and Parke (1964) pointed out that Schachter's aroused subjects first acted aggressively *and then* interpreted their feelings as "anger." Berkowitz (1965, pp. 321–322) took a somewhat similar position in attempting to explain how impulsive aggression might become purposive and goal-directed. Since the senior author's research has focused primarily on "impulsive" aggression in the past several years even though he has insisted that aggressors often intend to injure their victims, this conjectured relation between impulsive and goal-directed aggression might be summarized here.

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Briefly, Berkowitz hypothesized that aggressive desires may follow the aroused individual's interpretation of the *connection* between his internal reactions and some external event, object, or person rather than being a product of only his understanding of the nature of his feelings—the aspect emphasized by Schachter. Let us assume that a person is ready to act aggressively for one reason or another. Suppose he then encounters stimuli in his environment which evoke aggressive as well as other, correlated reactions from him. The responses, consisting of visceral, motoric, and ideational reactions, may be overt (such as a clenched fist or even an impulsive arm movement) as well as internal ones (including, possibly, ideas such as "What an S.O.B.!") Whatever their exact nature, the individual frequently becomes aware of them. He doesn't stop here, however. More often than not, he will also posit a connection between these reactions and something in his environment. He may believe that a certain person had this effect on him. The individual's particular interpretation of his internal responses is important, as Schachter has maintained. The individual thinks, for example, "I am angry," and this way of understanding his feelings facilitates the occurrence of aggressive behavior—among other reasons, by establishing an inhibitory "screen" which interferes with responses incompatible with aggression. But equally important, his interpretation also embraces a belief about the cause of his internal state: "*That* person made me angry." We hypothesize that an aggressive desire (or purpose) may then develop out of this understanding. The individual tells himself that he wants to hurt the person who had produced the reactions in him which he labels as "anger." He now has an intention, an aggressive goal, and may even become frustrated if he is unable to injure the person he now believes he wants to hurt.

Two recent experiments provide evidence consistent with this analysis. The first of these (Valins, 1966) has to do with cognitive effects on sexual preferences. When male subjects believed that their hearts had reacted markedly to certain slides of nude females, they rated these particular women as more attractive than the other nudes who supposedly had not influenced their heartbeats. They also expressed a greater preference for photos of the former women. Although influenced by Schachter's theory, Valins' findings seem more pertinent to the present conception. His men had particular sexual goals; they wanted certain photos more than others. The pictures they preferred were those they believed had affected them internally. Also dealing with the effects of perception of one's own reactions, Bandler, Madaras, and Bem (1968) demonstrated that (subjects') judgments of the painfulness of a stimulus were influenced by their observations of their own reactions to this stimulus.

But awareness of internal reactions does not always lead to goal striving. As the psychoanalytic theory of anxiety proposes, detecting the activation of socially disapproved responses within oneself may provoke anxiety and lead to a strong inhibition of these prohibited action tendencies. Clark (1952) has shown this type of inhibitory process at work. College men who were sexually aroused displayed fewer sexual themes in response to TAT cards than did a nonaroused control group, presumably because they had become aware of the socially disapproved sexual reactions within them and restrained all signs of these sexual responses. Sexual arousal did yield a high level of sexual themes, however, when the men were placed in a highly permissive setting.

Many forms of aggression are socially disapproved, much as the public expression of sexual desires is usually frowned upon. People in our society who believe that strong aggressive reactions have been evoked in them may restrain the open display of these disapproved actions, especially if they think that aggression is not warranted or proper in the situation confronting them. This brings up another aspect of the individual's understanding which also must be considered: his judgment of the appropriateness or propriety of aggression in the given situation. This type of judgment is probably affected by the nature and magnitude of the provocation he has suffered. If he had just experienced a moderate insult, he might not be disturbed at thinking he is moderately angry; the moderate provocation often justifies a moderate reaction. A very strong reaction to the moderate insult may well be improper and unwarranted, however, and therefore might give rise to anxiety and the inhibition of overt aggression.

The present subjects were all insulted moderately and then led to think that their tormentor had aroused either low, moderate, or high anger in them. "Anger" is here defined as the internal reactions, supposedly produced by the frustrater, which the individual either detects in himself or believes exist within him. (We usually refer to these reactions—which we experience in our viscera and limbs as well as in our thoughts—when we say that we feel angry.) Following the reasoning just outlined, we expect that the men who believe their tormentor made them moderately angry will attack this person more strongly than will the other subjects who think that the anger instigator has elicited only a low level of anger in them. It is as if the supposedly "moderately angry" people say to themselves, "Since he produced these moderately strong reactions in me, I must want to hurt him to a moderate degree. I *do* want to hurt him that much." The men led to believe that strong anger has been aroused in them, on the other hand, might think their strong reaction is not justified by the moderate insult they have suffered, could become anxious as a result, and then inhibit strong attacks on the anger instigator.

## METHOD

### *Subjects*

Forty-eight university men were distributed evenly among three conditions of the present experiment. Some had been recruited by ads placed in the student newspaper (which called for subjects for a psychological experiment and offered $1.50 in payment), while the others were volunteers from the introductory psychology classes who served in order to earn points counting toward their final grade. Subject source was controlled in assigning men to conditions so that source and experimental treatment were not confounded. Six other men had been "run" but were discarded before examining their data because they were highly suspicious. Four of these had been in the low-anger group and two in the high-anger condition.

### *Procedure*

When the subject came to the laboratory waiting room at the scheduled time, he found another male student also waiting for the session to start. This person, posing as a fellow subject but actually the experimenter's accomplice, quickly proved to be somewhat hostile and obnoxious. He began by expressing strong annoyance toward the tardy experimenter. Then, when the experimenter arrived to interview the two men before the session got under way, he made disparaging remarks about the University and also—by clear implication—the real subject.

The experimenter said that the purpose of the study was to investigate how certain physiological indexes varied in response to different types of tasks, such as learning and teaching tasks. Since the jobs to be assigned to them involved either giving or receiving very mild electric shocks, they were also informed that they could withdraw from the experiment if they so desired. None did, although a few inquired whether or not the shocks were really harmless. After these general instructions and at the conclusion of the brief interview (during which the confederate made a number of arrogant remarks), the experimenter assigned the two men to their respective roles, supposedly randomly. The accomplice was told that he would be the "learner" and would have to go to the next room, while the subject was designated the "teacher."

*Experimental manipulations.*[5] When the experimenter returned from ushering the other person to the next room, he explained that the other fellow, the "learner," was reading the instructions for his task. In the meantime, the experimenter went on, some physiological measurements would be taken from the subject so that his base-line levels would be known. The subject was informed that the apparatus on the table before him—an impressive-looking box covered with leads, switches, and dials—supposedly would record his blood pressure, heart rate, and galvanic skin response and then integrate the readings to obtain psychologically meaningful scores. Various wires were then attached to the subject. It was explained that these were only sensing devices and that the subject would not feel anything from them. He was also given a data sheet.

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[5] Uncritically accepting the image of the highly suggestible and cooperative subject inherent in "demand characteristics" interpretations of experimental results, we had thought that subjects might become relaxed and uninhibited if they were led to think that they had taken alcohol. Forty-eight other subjects drank a concoction supposedly containing alcohol and were told that they would feel at ease and relaxed. However, instead of exhibiting unrestrained aggression, the high-anger subjects receiving this supposed alcohol treatment appeared to be even more inhibited than their counterparts in the no-alcohol condition. Thus, where the shock-intensity data for all 96 subjects yielded a highly significant interaction of anger intensity by trial blocks ($F = 3.1$, $df = 6/270$, $p < .01$), very similar to the pattern shown in Table 2, the greatest anger-intensity level differences were in the no-alcohol group. In accord with our initial hope, the alcohol subjects rated themselves as less sluggish than the no-alcohol group, but the alcohol subjects also described themselves as more self-centered and as enjoying the experiment less. Moreover, the high-anger–alcohol group had the *lowest* self-ratings of all six groups on the items "cocky," "light-hearted," and "affectionate," significantly lower than those in the high-anger–no-alcohol group. This alcohol condition is omitted in this report because of the ambiguities in interpreting the findings.

Immediately after this the experimenter said that further instructions were on recording tape in order to save time. The tape recorder was turned on and the experimenter left, ostensibly to see how the other person was doing with his own assignment.[6]

The taped voice directed the subject's attention to a meter featured prominently on the "psychoautonomic integrator." Called the "anger meter," this device employed a 6-point scale, 0–5, and had labels at three levels indicating "low," "medium," and "high" anger. The voice instructed the subject to think of certain people, objects, and concepts. As he thought of each, he was to look at the meter reading and write down the value on the data sheet. These readings, the subject was told, would provide the base-line data assessing his mood, "such as anger." The various concepts were then called off one at a time: home state, field of psychology, favorite professor, other subject in the experiment, experimenter. The dial reading for each of these concepts with the exception of the next to the last (other subject), which was actually controled from the other room, always fluctuated below 1, signifying very low anger.

The experimental conditions differed in the level of anger the subject supposedly felt toward his partner. In the low-anger condition, the meter reading fluctuated between .5 and 1, went from 2.25 to 2.75 in the medium-anger group, and varied between 4 and 4.25 in the high-anger condition.

*Dependent variables.* As soon as the supposed base-line readings had been taken, the taped voice told the subject that his partner would try to learn a code using information given to him. Following the procedure employed by Buss (1961), it was also explained that the subject was to give his partner electric shocks whenever that person made a mistake. The subject was to use the other apparatus on the table in the teaching task. One set of lights on this apparatus (the Buss "aggression machine"), marked "right" and "wrong," would indicate whether or not the learner's response on the given trial had been correct. He was to be shocked whenever the lights showed that he had made a mistake. The teacher's (i.e., the subject's) task was to select the intensity of the shock that would be best in teaching the other person that his answer was incorrect. Ten buttons were located on the apparatus. By pushing one of these, the subject could administer a shock ranging from very low (Button 1) to a much stronger, although nonharmful, intensity (Button 10).

The partner's responses were "wrong" on 14 of the 20 trials. Mean shock intensity scores were obtained for each of four blocks of trials: 1–4 and 5–8 (before there was any "right" answer), 10–12, and then Trials 14, 15, 17. No effort was made to record the duration of the shock given on each critical trial, and so only the total duration of the shocks given by the subject over all 14 critical trials was recorded. Three aggression measures serve as the dependent variables: (*a*) mean shock intensity in each trial block, (*b*) total duration over all trials, and (*c*) a composite score. In accord with Hartmann's (1965) suggestion, this composite index was obtained by multiplying the mean intensity of all the shocks given by the subject by the total duration of these shocks.

The experimenter returned at the completion of the learning task and administered a mood questionnaire. The first half was the 24-item Nowlis Mood Questionnaire (Nowlis, 1965), which consisted of eight 3-item clusters. In responding to each item, the subject was to check the extent to which the given adjective characterized his present mood on a 5-step scale ranging from "definitely applies to "definitely does not apply." The second half was an unvalidated questionnaire which attempted to control for the social desirability of the response. Since no significant results were obtained with this latter instrument, there will be no further mention of it.

When the subject had finished filling out the mood measures, the experimenter undertook a standardized interview designed to assess whether the subject had been suspicious, and if so, how specific these misgivings were. As will be reported later, the conditions did not differ in the level of doubt created in the subjects. At the conclusion of this interview, the experimenter explained the deceptions that had been practiced and the reasons for them, and asked the subject not to talk about the study for the remainder of the semester.

## RESULTS

### *Aggressive Behavior*

As shown in Table 1, the information given the subject about his level of anger toward his obnoxious partner had a significant effect on the intensity of shocks he administered to this person. There was a significant main effect for the anger-intensity treatment as well as a significant interaction of anger intensity by trial blocks. Looking at Table 2, we see that the men in the medium-anger group were reliably more punitive toward the "learner" than were the subjects in each of the other two conditions. The people in the high-anger group, on the other hand, did not give more intense shocks than the subjects in the low-anger condition, presumably because they had become anxious and inhibited strong aggression.

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[6] There were two experimenters (John P. Lepinski and Eddy J. Angulo, the latter obviously a foreigner) and three confederates, with each experimenter and each confederate appearing the same number of times in each condition. Of the six subjects discarded, only one had been run by the "foreigner." Analysis of the results indicated, nevertheless, that no systematic results were attributable to the different experimenters or confederates, and these sources of variation are ignored in this report.

Other results shown in the table also point to comparatively strong aggressive inclination (or purposiveness) in the medium-anger condition. The low-anger subjects administered somewhat less intense punishment to their partner in the last two trial blocks than in the second block—they eased up on him once he began to give right answers. This decline was not significant, however. The supposedly very angry subjects generally did not show any decline in shock intensity whatsoever after the second block of trials, but also did not become more openly aggressive as the trials progressed. By contrast, the men thinking themselves moderately angry became significantly more punitive in the final block, as compared with the preceding trials, even though the "learner" had begun to give correct answers. They evidently wanted to hurt this person and were not going to let his increasingly correct responses prevent them from doing so.

TABLE 1

ANALYSIS OF VARIANCE OF MEAN SHOCK INTENSITY

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Anger intensity (A) | 2 | 49.39 | 3.33* |
| Error | 45 | 14.86 | |
| Trial blocks (B) | 3 | 37.23 | 24.81** |
| A × B | 6 | 3.58 | 2.38* |
| Error | 135 | 1.50 | |

* $p < .05$.
** $p < .001$.

The shock-duration measures also revealed the curvilinear relation between perceived anger intensity and the strength of the subsequent aggression. As Table 3 indicates, the subjects in the medium-anger condition generally gave longer shocks than the men in either of the other groups. The subjects in the high-anger condition administered somewhat (but not reliably) longer punishment than the people in the low-anger group, so the medium-anger subjects were significantly different from the men in the low-anger condition only. The overall aggression index, consisting of mean intensity times total duration, led to the same pattern of findings, as would be expected. With this measure, however, the medium-anger condition was significantly more aggressive than either the low- or high-anger groups, and these two latter conditions did not differ from each other.

TABLE 2

MEAN INTENSITY OF SHOCKS IN THE FOUR TRIAL BLOCKS

| Anger-intensity conditions | Trial blocks | | | | *M* over all 14 trials |
|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | |
| Low | $3.52_{mM}$ | $4.92_{mN}$ | $4.38_{mN}$ | $4.40_{mN}$ | $4.30_a$ |
| Medium | $4.16_{mM}$ | $6.13_{nN}$ | $6.21_{nN}$ | $7.21_{nP}$ | $5.92_b$ |
| High | $3.19_{mM}$ | $4.97_{mN}$ | $4.90_{mN}$ | $5.06_{mN}$ | $4.53_a$ |
| *M* over intensity conditions | $3.62_x$ | $5.34_y$ | $5.16_y$ | $5.56_y$ | |

Note.—Separate Duncan range tests were performed for each of the three terms, the two main effects, and the interaction. Cells having a subscript in common are not significantly different at the .05 level. In the comparisons of the means for each trial block in the interaction, small letter indicates vertical comparisons (among conditions within a given trial) and capital letters indicate horizontal comparisons. $n = 16$ per cell.

TABLE 3

CONDITIONS MEANS ON SHOCK DURATION SCORES

| Measure | Anger condition | | | *F* ratio |
|---|---|---|---|---|
| | Low | Medium | High | |
| Total duration (in sec.) | $10.1_a$ | $16.2_b$ | $12.4_{ab}$ | 4.80* |
| *M* Intensity × Total Duration | $45.5_a$ | $98.7_b$ | $57.7_a$ | 6.06** |

Note.—Separate analyses were made for each measure. Cells having a subscript in common are not significantly different at the .05 level by Duncan multiple-range test.
* $p < .05$.
** $p < .01$.

### *Mood Responses*

Findings obtained with the Nowlis Mood items support our explanation of the low level of aggression in the high-anger condition. Six measures yielded significant main effects. The five shown in Table 4 suggest that the men

TABLE 4

Mean Condition Scores on Mood Items Suggestive of Anxiety

| Item | Anger conditions | | | $F$ ratio |
|---|---|---|---|---|
| | Low | Medium | High | |
| Sluggish | $1.94_a$ | $2.00_a$ | $3.19_b$ | 5.29*** |
| Drowsy | $1.88_a$ | $1.81_a$ | $2.88_b$ | 3.47** |
| Tired | $1.75_a$ | $2.13_a$ | $3.31_b$ | 7.92*** |
| *M* of above items | $1.85_a$ | $1.98_a$ | $3.12_b$ | 7.92*** |
| Insecure | $2.25_{ab}$ | $1.63_a$ | $2.75_b$ | 3.19* |

Note.—The above scales run from 1 (definitely does not apply) to 5 (definitely applies), so higher values indicate that the subject considered the quality more characteristic of himself. Cells having a subscript in common are not significantly different at the .05 level by Duncan range test.

* $p < .06$.

** $p < .05$.

*** $p < .01$.

who were led to believe that they were very angry toward their partner subsequently felt more anxious than the people in the other two conditions. Thus, they rated themselves as reliably more sluggish, drowsy, and tired than the other subjects. Not surprisingly, when the mean of these three items was obtained, forming an index proposed by Nowlis, there were the same results. In addition, the high-anger subjects also rated themselves as reliably more insecure than the people who were told that they were only moderately angry. All in all, the men presumably thinking themselves to be inappropriately highly angry seemed to experience the feelings frequently associated with neurasthenic anxiety.

The findings obtained with the sixth item (not shown in Table 4) are somewhat more difficult to interpret. There was also a significant main effect for the rating of felt anger ($F = 3.71$, $df = 2/45$, $p < .05$), and again the high-anger men rated themselves as having more of this particular mood than the subjects in the other conditions. What are puzzling, however, are the virtually identical means for the low- and medium-anger groups. Why did the people in the latter condition not rate themselves as angrier than the subjects in the low-anger group? The measure might not have been sufficiently sensitive. Also, even though there was no systematic relationship between prior aggression and rated anger *within* the condition, the high volume of open aggression previously displayed by the medium-anger subjects conceivably might have led to the lower ratings. This last could be a "true" lowering of felt anger because of a hostility catharsis, or only a reluctance to admit feeling angry because of guilt and/or anxiety created by the medium-anger subjects' strong prior attacks on their partner.

### *Suspiciousness*

We must now confront an alternative explanation for the present results. Rather than taking the meter readings as information about their own anger level, the subjects might have suspected that the experimenter wanted them to act in accord with the reported anger level; the meter supposedly presented the "demand characteristics" of the experiment. As popular as this type of notion is nowadays, a variety of considerations argue against this simple "demand" explanation.

Several psychologists have noted, for example, that subjects generally seem to be more concerned about "placing their best feet forward" and giving the experimenter a good impression of themselves than about pleasing the researcher by confirming his hypothesis. They are presumably guided more by evaluation apprehension than by demand characteristics. Sigall, Aronson, and Van Hoose[7] recently have provided clear evidence for this contention. Indeed, the comparatively weak aggression but relatively strong anxiety displayed by the high-anger men can readily be understood in evaluation apprehension terms; fear of what the experimenter might think of their strong anger toward their partner might have contributed to their general anxiety.

The intensive probing of the subjects at the end of the experimental session also did not support the demand-characteristics explanation. Although most of the subjects' suspicions were directed toward the meter readings rather than toward the confederate, the conditions were almost identical in number of subjects expressing doubts about the meter and/or the confederate, and in the rated mean intensity of these suspicions. There are no indications, then, that the subjects were more aware of the investigators' hypothesis in one condition (say the medium-anger group) than in the others.[8]

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[7] Unpublished study entitled "The cooperative subject: Myth or reality?" 1968.

## DISCUSSION

In the absence of the required control group, we cannot say whether the anger-level information increased the medium-anger subjects' inclination to attack their partner, lowered this inclination in the low-anger group, or did both. Whatever the direction of the effect, our findings suggest that the aroused men were generally motivated to act in accord with their understanding of their emotional state—as long as this state was appropriate to the situation.

This matter of appropriateness, of course, is pertinent to the low level of aggression displayed by the men in the high-anger group. As suggested earlier, these people presumably became anxious because they believed their strong anger was unwarranted in this particular situation and consequently restrained their attacks upon their partner. Three points could be made here regarding this contention. First, where other items in the Nowlis Mood Questionnaire (such as "insecure" or "sad") appear to tap "anxiety" more directly than the fatigue-like measures shown in Table 4, the diffuse fatigue reported by many of the high-anger subjects is probably indicative of anxiety. Anxiety, clinically, is not always manifested in specific feelings of worry, or explicit self-doubt or self-blame. Indeed, it is often experienced as something vague and unfocused, as in "free-floating anxiety," especially when the individual is unaware of, or unwilling to admit, the source of his discomfort. If the high-anger men wanted to deny their anger to themselves and/or to the experimenter, they would also not care to say that they had anything definite to be worried about. They could acknowledge a more diffuse discomfort, however. The feelings they did report, moreover, are clinically meaningful; they are strikingly similar to symptoms of neurasthenia, a syndrome characterized by fatigue and lassitude (cf. English & Finch, 1957, p. 324). In this connection, interestingly enough, English and Finch (1957) have described neurasthenic people as "investing a great deal more of their mental energy on their inner physiological processes than they are in the world about them [p. 324]." The high-anger subjects may also have been attentive to "their inner physiological processes," thinking that they possessed undesirably strong anger towards the confederate.

The second point we wish to raise has to do with the time at which the mood scales were administered. We obviously would have had clearer evidence of the role of anxiety in inhibiting overt aggression by the high-anger subjects if the mood reports had come before the shock opportunity. However, a growing body of research has shown that questionnaire ratings often exert a substantial, and sometimes very complicated, influence on subsequent aggression (see, for example, Mallick & McCandless, 1966). Since our primary interest was in the effect of our manipulations on the subjects' overt behavior, this preliminary investigation deliberately focused on behavioral measures uncontaminated by earlier ratings. All this aside, however, consider what could be said about the results in the high-anger condition. We can say either that the low level of aggression was caused by the anxiety reflected in this group's mood scores, or that the anxiety was the product of the low aggression. The former alternative is clearly more plausible.

Our final point is also related to clinical experiences. Some might object that the high-anger subjects should not have described themselves as anxious, tense, or even fatigued, since they had not attacked their partner too strongly. What did they have to be anxious about? the critic asks. The clinical literature abounds in refutations of such an objection. Many people have become anxious enough to seek therapeutic help merely because they had socially disapproved ideas and inclinations, although they had never acted on these thoughts and feelings. Parents have requested psychiatric aid because they believed they had unnatural and immoral urges (aggressive or sexual) toward their children, while men have panicked at recognizing a homosexual desire in themselves. The ideas, feelings, or wishes had not been consummated in these cases, but awareness of them had nevertheless produced intense anxiety.

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[8] The apparent tension experienced by the subjects in the discarded alcohol condition also is contrary to a simple "demand" explanation. In spite of the suggestion that they would feel relaxed, they acted as if they were tense, anxious, and inhibited.

Accepting the present results at face value, then, the findings are relevant to contemporary cognitive theorizing in social psychology. In line with Bem's contention that self-observation can affect subsequent action (cf. Bandler et al., 1968), they show that an individual's behavior toward some external stimulus is often influenced by his perception of his initial responses to that stimulus. Festinger's (1957) analysis of the spread of rumors is essentially in the same vein. Rumors spread, he maintained, when people are (*a*) aware that they have been frightened by some external event, but (*b*) regard these implicit fear reactions as unwarranted or inappropriate in the given situation. Communications to others about the external threat supposedly arise in order to justify the felt anxiety.

Festinger's reasoning also assumes, of course, that the overt behavior regarding the external stimulus (talking about the threat, in this case) tends to become consistent with the perceived internal emotional reactions. The present findings can also be understood in this manner. Within the limits of judged safety, appropriateness, and propriety, we apparently want to act in a way that is consistent with our conception of ourselves. We may act in a socially "bad" fashion if we think badly of ourselves, as Aronson and Mettee (1968) have hypothesized. We behave aggressively or excitedly if we are induced to think of our ambiguous internal sensations as "anger" or "euphoria," according to Schachter's (1964) research. And evidently, as the present paper maintains, we are inclined to attack those persons who we believe have evoked the internal reactions we label as "anger."

This line of thought can also resurrect a modified version of the classic James-Lange theory of emotions. William James had suggested, it will be recalled, that specific emotions arise from the perception of bodily changes. Cannon subsequently cast this theory into limbo by showing the inadequacy of equating emotions with visceral changes, but cognitive theorizing can overcome at least part of Cannon's critique (cf. Schachter, 1964). There may well be a large kernel of truth in the James-Lange theory despite Cannon's objections. Alterations, nevertheless, have to be made. Among other things, we may have to recognize that the aroused individual detects more than visceral changes; he may also be aware of his muscle and skeletal reactions, the ideas that come to mind, and also the connection between these reactions and some external person, object, or event.

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# AN ANALYSIS OF ACTIVE AND PASSIVE DEFENSES IN INDUCING RESISTANCE TO PERSUASION

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A $2 \times 2 \times 2$ factorial experiment was performed to examine the interaction of active versus passive refutational defenses and immediate versus delayed attacks upon resistance to persuasion. As predicted, following active defenses a delay in the attack increased resistance to persuasion, while following passive defenses a delay in the attack decreased resistance. A similar interaction effect was found on a dependent variable indicative of tendencies to discount the attacking message. Contrary to inoculation theory, there was no evidence that accumulation of belief-bolstering arguments produced the interaction effect upon opinions. An explanation is offered for the observed interaction effects which uses concepts from both the inoculation and congruity formulations.

McGuire's inoculation theory of inducing resistance to persuasion concerns the creation of immunity to an attack on a cultural truism by defensive pretreatments. According to McGuire (1964), effective defensive pretreatments typically have (*a*) a threatening component which produces realization of the vulnerability of the belief in the cultural truism and which supplies motivation to acquire belief-bolstering arguments, and (*b*) an informative component which provides the required arguments. He further assumed that when only the first component is effectively presented, the believer will, if given sufficient time, accumulate belief-bolstering arguments through his own efforts. In earlier formulations of the theory (McGuire, 1961a, 1961b; Papageorgis & McGuire, 1961) the potential importance of a third component of a defensive pretreatment, which Tannenbaum (1967) called the assertion-weakening component, was acknowledged; in later presentations of the theory this characteristic of defenses was not emphasized, perhaps because it does not fit the biological analogy used to describe the theory.

The evidence showing that subjects tend to assimilate belief-bolstering material when it is presented in conjunction with a threat is fairly convincing (McGuire, 1961a, 1964; McGuire & Papageorgis, 1961, 1962), but the evidence showing that a threat will stimulate subjects to acquire, through their own efforts, such belief-bolstering arguments is much less impressive. The most direct test of this hypothesis was reported by Papageorgis and McGuire (1961). They predicted that subjects who had been exposed to a refutational defense would be stimulated, during the week following this experience, to think up more arguments in support of their belief. Thus they predicted that when subjects are asked to list arguments favoring the initial belief on each truism, those who had been exposed to immunization 1 week earlier would be able to list more supportive arguments than those who had not been exposed to immunization. Although the difference in the mean number of arguments listed was in the expected direction, the difference was insignificant. However, when students were asked to rate the quality and credibility of the attacking message, there was a significantly greater degree ($p < .01$) of derogation of this message exhibited by subjects previously exposed to immunization than by those not exposed to immunization. Thus these results support the potency of the assertion-weaking component, rather than the defense-alerting component emphasized subsequently by McGuire (1964). Macaulay (1965), using different materials, also found that subjects exposed to a refutational pretreatment rated an attacking message significantly lower ($p < .01$) with respect to fairness and believability than subjects not exposed to such a pretreatment. Moreover, she found no differences between these groups with respect to their expressed interest in receiving more information on the truism.

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Tannenbaum (1967) has recently argued that "in most of the experiments cited by McGuire, the assertion-weakening characteristic of the (defensive) refutation would account for the obtained data just as readily as does the threat-providing aspect underlined by inoculation theory [p. 293]." Perhaps the results most difficult to account for in terms of the message or source disparagement factors stressed by Tannenbaum are the delayed-action effects of active defenses. McGuire (1964, pp. 225–227) has described an experiment in which active defenses produced delayed action effects over a 1-week interval. More specifically, McGuire reported that following active defenses an increase in the delay of the attack produced increased resistance to attack, while following passive defenses an increase in the delay of the attack produced decreased resistance. These results may be readily explained by supposing that the experience of being unable adequately to defend one's belief in the truism stimulated subjects in the active condition to accumulate belief-bolstering arguments during the week that followed, and as a consequence they exhibited greater resistance to the delayed attack. In contrast, it is not apparent why assertion-weakening tendencies among subjects exposed to active defenses should be stronger after a delay of 1 week than at the time of the immediate attack. Thus these results appear to be more readily accounted for on the basis of the threatening component than upon the basis of the assertion-weakening component of defensive pretreatments. However, McGuire does not report any comparisons between immediate and delayed treatments with respect to (*a*) ability to list arguments in support of the truism, or (*b*) tendencies to discount the attacks. As a consequence, the data for evaluating these rival explanations are not highly persuasive.

The major purpose of the present experiment was twofold: first, to replicate McGuire's reported finding of an interaction effect between active versus passive refutational defenses and immediate versus delayed attacks; and, second, to obtain measures of each subject's discounting tendencies and of his related cognitive repertory in order to assess rival explanations of the effect. Specifically, it was expected that if a similar interaction effect could be demonstrated for a variable indicative of the number of arguments the subject could produce in support of the truism, then McGuire's belief-bolstering interpretation would be supported. On the other hand, a similar interaction effect upon a measure indicative of the subjects' discounting tendencies might be interpreted as a simple generalization effect resulting from differential opinion change. A rival explanation might be sought in terms of McGuire's and Tannenbaum's alternative interpretation having to do with differential assertion-weakening tendencies, though it is not clear how a delayed-action effect upon discounting tendencies could be explained by congruity principles.

## METHOD

### *General Procedure*

The experiment was presented to subjects as an attempt to develop a test of analytic intelligence for the United States Department of Health, Education, and Welfare. The project allegedly was investigating the relationships between performance on such a test and measures of personality characteristics and of attitudes towards the test materials. Subjects were told that the study was being conducted at several large universities, and that it was therefore necessary to conduct the experiment in a standardized manner. In keeping with this story, the experimenter used a stopwatch to insure that subjects observed the given time allotments, and instructed subjects that they were not to talk or ask additional questions after the experiment had begun.

*Design.* A $2 \times 2 \times 2$ factorial design with after-only measures on the dependent variables was used with two between-subjects experimental manipulations: passive versus active defenses and immediate versus delayed attacking messages. The third factor was a repeated-measures factor and consisted of the specific health truism on which defenses, attacks, and dependent measures were presented. In addition, the customary control conditions were included: namely, an attack-only condition and a neither-defense-nor-attack condition. Experimental subjects in the immediate-attack condition participated in two consecutive 1-hour sessions, while those in the delayed-attack condition participated in two 1-hour sessions separated by a 2-day interval. The first session of each experimental condition was designed to expose the subject to one of two types of defensive pretreatments; the second session exposed him to messages attacking the subject's beliefs concerning each of two health truisms. The latter were identical to two used in most of McGuire's experiments: "Everyone should get a chest X ray each year in order to detect any possible tuberculosis symptoms at an early stage"; and "The effects of pencillin have been, almost without exception, of great benefit to mankind."

*First experimental session.* After reading an instruction sheet developing the cover story and asking preparatory questions of the experimenter, subjects completed the Gordon Personal Inventory. Half of the subjects were then instructed to read a refutational defense of one of the two health truisms and to underline the key phrase in each paragraph. After 8 minutes the subjects were given a second refutational defense on the other health truism and asked to read it, and underline key phrases as before, during a second 8-minute period. Each defense consisted of a five-paragraph essay of approximately 1,000 words. The initial paragraph of each essay stated the health truism and mentioned four one-sentence counterarguments sometimes given in opposition to the health practice expressed in the truism; the subsequent paragraphs each restated and refuted one of the counterarguments in a detailed, factual manner.

The remaining half of the subjects were exposed to active defensive pretreatments on the same two truisms and on the same counterarguments. Following completion of the personality inventory, each of these subjects was given two pages on which were listed four counterarguments critical of one of the truisms, and asked to write in the spaces provided the best arguments he could think of to refute each objection. After 18 minutes, the subject was given two additional pages on which were listed four counterarguments critical of the other health truism, and asked to write refutations of each argument during a second 18-minute period.

The defense materials were similar to those used by McGuire (1962), except that four, rather than two, counterarguments were included in each type of defense. For example, the four-argument messages in the passive defense were simply synthesized from the two-argument messages used by McGuire in his refutation-same and refutation-different treatments. It was expected that this modification would increase the threatening and motivating characteristics of the active defenses. Since the counterarguments mentioned in the defenses were the same as those presented and developed in the subsequent attacking messages, these defenses corresponded to the type previously called refutational-same defenses by McGuire.

*Second experimental session.* Half of the experimental subjects were exposed to the attacking messages immediately after exposure to the defenses, while the remaining half were exposed to delayed attacks which came 2 days after the initial session. Subjects who were to return in 2 days were dismissed after the first session and asked not to discuss the experiment with anyone. In the second session each subject was given 8 minutes to read the attacking message on one of the health truisms and to underline the key phrase in each paragraph. The attacking message on the second truism was then introduced and an additional 8 minutes were given to read and underline key phrases in this message. A posttest of opinions on each truism was next administered, purportedly to determine whether attitudes toward the content of the verbal materials had affected performance on the test of analytic intelligence. Copies of the attacking messages were then given to the subjects a second time, and subjects were permitted to refer to them as they completed a reaction questionnaire concerning the factual adequacy and fairness of each attacking message. Each subject was then asked to list briefly as many independent arguments as he could think of which could be developed as a defense of the proposition expressed by the first truism. After 10 minutes, he was asked to list similar arguments for the second truism during a second 10-minute period. Papageorgis and McGuire (1961) reported that in their earlier experiment some subjects complained that the time allowed for listing arguments (5 minutes) was inadequate and the time allotment for this phase of the experiment was doubled. The final exercise in this session consisted of completing a brief postexperimental questionnaire.

In the two experimental sessions the order of presentation of truisms was counterbalanced within each treatment combination. However, if a subject received the defensive pretreatments in, say, the "penicillin-X ray" order, then the attacking messages and dependent measures were presented in the same order.

*Control session.* Control treatments required less than an hour to administer. The standard cover story was administered first. In the attack-only condition subjects were next asked to read the attacking messages on each truism, and to underline key phrases in each paragraph, while in the neither-defense-nor-attack condition this step was omitted. Subjects were then given the posttest measure of opinion, the reaction questionnaire (given only to subjects in the attack-only condition), the request to list as many arguments as possible in support of the truism, and the postexperimental questionnaire. For each of the overlapping exercises the time allotments used for the experimental subjects were observed.

At the completion of the data collection, all subjects were informed of the true purpose of the experiment and cautioned that the arguments had been selected solely for persuasive purposes and that no credence should be given to any of the arguments simply because they had been used in the experiment.

### *Subjects*

The subjects were 108 students at Vanderbilt University enrolled in the introductory psychology course, who served in the experiment as a means of satisfying a course requirement. Subjects were run in groups of 6 in order to facilitate the data collection. No more than 2 subjects were recruited for a given hour from the same section of the course, and each block of 6 subjects was assigned randomly to one of the four between-subjects experimental cells (18 subjects assigned to each of the four between-subjects conditions). In recruiting subjects, the experiment was described as one which required either two consecutive 1-hour sessions or two 1-hour sessions separated by a 2-day interval, and it was indicated that subjects would be told at the first experimental session in which of the two arrangements they would serve. Control subjects were recruited for a single hour session, and each block of 6 subjects was assigned randomly to one of the two control conditions.

### *Dependent Variables*

*Posttest measure of opinion.* Beliefs on the two truisms were measured by a nine-item opinion questionnaire. Each item consisted of a statement (e.g., "Everyone should get a chest X ray each year in order to detect any possible TB—tuberculosis—symptoms at an early stage") followed by a 15-point graphic rating scale, labelled Definitely False at one end and Definitely True at the other. The subject was instructed to circle the number on the scale best indicating his judgment of the truth of the statement. There were four items on each truism, and the items were identical to those previously used by McGuire (1962.)[2] The statements were varied so that on some of the items 15 represented complete agreement with the truisms, and on others 15 represented completed disagreement. In scoring the responses, ratings were converted so that 15 always indicated complete agreement with the truism. The scores presented in the Results section are based on the mean of the items on each truism. One of the nine items was repeated to provide a reliability estimate of each item response; the responses to the repeated item yielded an overall correlation of .93.

*Discounting tendencies.* To assess the extent to which assertion-weakening components of the defenses were operative, the subjects were encouraged to review the attacking messages and asked to evaluate (on the reaction questionnaire) each attacking message on six evaluative items: "All of the important facts were taken into account in the excerpts"; "The excerpts did a good job of giving the facts"; "The conclusions reached were justified by the facts presented"; "An insufficient amount of evidence was presented"; "The excerpts evidence a clear understanding of the relevant facts"; and "The facts were exaggerated to prove a point." Each of these items was rated on a 10-point graphic scale, with a rating of 10 indicating high disparagement for some items and low disparagement for others. Ratings were converted in scoring responses so that 10 always represented high disparagement. Scores presented in the Results section are based upon the mean of the six ratings.

*Number of favorable arguments.* Each subject was asked to list as many independent arguments as he could think of that could be developed as a defense of the truism. The senior author and a graduate student independently rated numbers of different arguments listed, under conditions in which each rater was blind with respect to the subject's treatment condition. The average correlation for the judged number of arguments listed for the two truisms was .85 for the two raters. The scores used in the analysis of results were the average ratings based upon both raters.

## RESULTS

### *Effectiveness of Defenses in Producing Resistance*

The footnote to Table 1 shows that the mean opinion level (averaging over the two truisms) for subjects in the neither-defense-nor-attack group was 12.45, indicating that subjects initially expressed a high degree of endorsement of the two propositions. In contrast, the corresponding mean opinion level for subjects in the attack-only condition was 7.07. These two control-group means differed at a high level of significance ($p < .001$). Table 1 also shows that the average opinion levels for subjects in each of the four experiment groups fell between the mean levels for the two control groups; all of these means (except that for the active defense, immediate-attack condition) differed at the .05 level or higher from the mean for the attack-only group, while only the two active defense means differed at the .05 level or higher from the neither-defense-nor-attack condition.[3]

### *Effects of Independent Variables on Resistance*

Table 2 summarizes the results of analyses of variance of each dependent variable. It can be seen that the defense manipulation had a highly significant main effect upon opinions ($p < .001$); Table 1 shows that the passive defenses generally produced greater resistance than the active defenses.

The main prediction of an A × B interaction effect upon opinions was also confirmed

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[2] McGuire's questionnaire items and his defensive and attacking messages (from which the messages were constructed) have been deposited with the American Documentation Institute. Order Document No. 7058 from ADI Auxiliary Publications Project, Photoduplication Service, Library of Congress, Washington, D. C. 20540. Remit in advance $5.00 for photocopies or $2.25 for microfilm and make checks payable to: Chief, Photoduplication Service, Library of Congress.

[3] All significance levels reported are based upon one-tailed tests of significance.

TABLE 1

MEAN SCORES ON THE OPINION, DISCOUNTING, AND LISTING MEASURES

| Dependent variable | Issue | Passive defense | | Active defense | |
|---|---|---|---|---|---|
| | | Immediate | Delayed | Immediate | Delayed |
| Opinion[a] | Penicillin | 12.71 | 12.64 | 11.00 | 12.29 |
| | X ray | 11.08 | 10.00 | 6.44 | 8.19 |
| | Average | 11.89 | 11.32 | 8.72 | 10.24 |
| Discounting tendency | Penicillin | 6.82 | 5.97 | 4.68 | 5.79 |
| | X ray | 7.24 | 6.01 | 5.11 | 5.50 |
| | Average | 7.03 | 5.99 | 4.90 | 5.64 |
| Arguments listed | Penicillin | 3.97 | 4.11 | 3.44 | 3.31 |
| | X ray | 4.14 | 3.92 | 3.14 | 3.42 |
| | Average | 4.06 | 4.01 | 3.29 | 3.36 |

[a] Subjects in the attack-only condition had mean opinion scores of 9.32 and 4.81 on the penicillin and X-ray issues, respectively; subjects in the neither-defense-nor-attack condition had mean opinion scores of 12.57 and 12.32 on the penicillin and X-ray issues, respectively.

($p < .05$), thus replicating McGuire's finding. Table 1 shows that following active defenses a delay in the attacking message increased resistance to the attack, while following passive defenses a delay in the attack slightly decreased resistance to the attack. Within the active condition the mean of 10.24 for the delayed group differed significantly from the mean of 8.72 for the immediate group ($p < .05$), indicating a genuine delayed-action effect upon opinion. Within the passive condition, the overall means for the immediate and delayed groups did not differ significantly.

There was also a highly significant main effect ($p < .001$) associated with the specific truism used: without exception the defensive pretreatments produced greater resistance to the attacking message on the penicillin topic than to the message on the X-ray topic. Finally, the significant A × C interaction ($p < .01$) between type of defense and truism indicates that the superiority of the passive over the active defense in conferring resistance was greater on the X-ray issue than on the penicillin issue.

## *Discounting Tendencies*

Table 2 shows that type of defense had a significant main effect upon discounting tend-

TABLE 2

ANALYSIS OF VARIANCE FOR THE MEASURES OF OPINION, DISCOUNTING TENDENCY, AND ARGUMENTS LISTED

| Source | Opinion | | Discounting tendency | | Arguments listed | |
|---|---|---|---|---|---|---|
| | *MS* | *F* | *MS* | *F* | *MS* | *F* |
| Between *S*s | | | | | | |
| Type of defense (A) | 162.138 | 18.04*** | 55.440 | 10.99** | 18.063 | 12.24*** |
| Delay (B) | 8.123 | <1 | .774 | <1 | .007 | <1 |
| A × B | 39.166 | 4.36* | 28.775 | 5.71* | .111 | <1 |
| *S*s within groups | 8.988 | | 5.044 | | 1.476 | |
| Within *S*s | | | | | | |
| Messages (C) | 376.684 | 83.45*** | .835 | <1 | .112 | <1 |
| A × C | 43.120 | 9.55** | .224 | <1 | .062 | <1 |
| B × C | .666 | <1 | 2.775 | 2.51 | .006 | <1 |
| A × B × C | 4.733 | 1.05 | .256 | <1 | 1.362 | 1.50 |
| C × *S*s within groups | 4.514 | | 1.106 | | .907 | |

* $p < .05$, $df = 1/68$.
** $p < .01$, $df = 1/68$.
*** $p < .001$, $df = 1/68$.

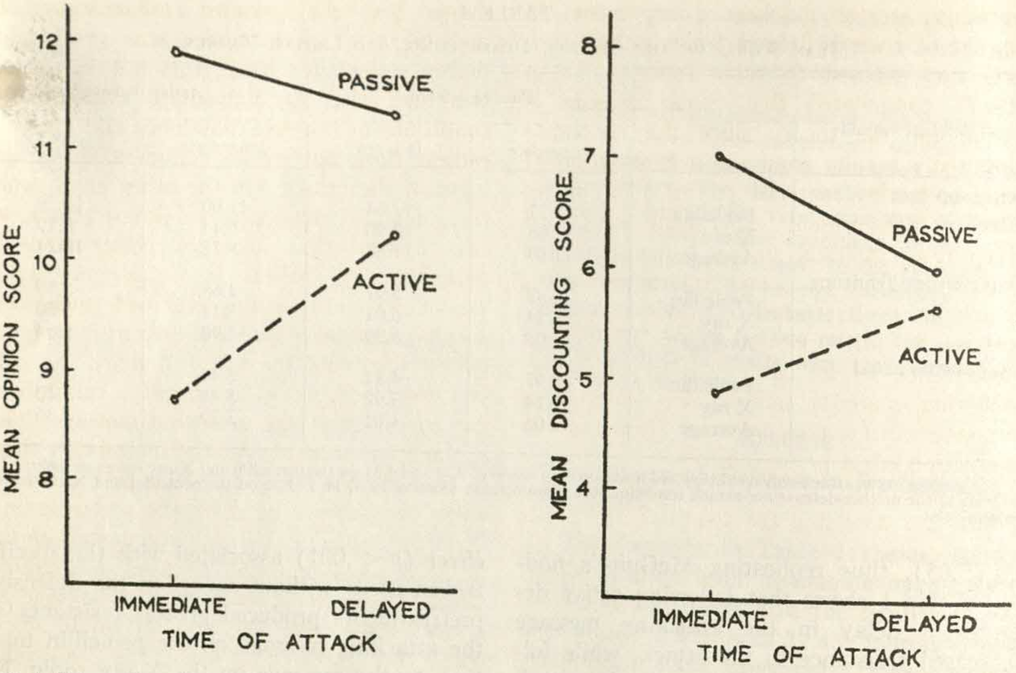

FIG. 1. Interaction effects of type of defense and time of attack upon resistance and discounting measures.

encies, with the passive defenses producing greater disparagement of the attacking messages than the active defenses (Table 1). In addition, there was a significant Defense × Time of Attack interaction effect upon discounting tendencies ($p < .05$). An examination of the cell means indicates that on each truism the superiority of the passive over the active defense in producing discounting of the attacking message was greater for the immediate attacks than for the delayed attacks. Orthogonal comparisons show that in the immediate attack condition the passive defense produced a significantly greater degree of disparagement of the attack than the active defense, while the two defenses did not differ in the delayed-attack condition. Also among subjects receiving active defenses, the average disparagement of the delayed attack did not differ significantly from that of the immediate attack. There was no statistically significant evidence of a sleeper effect upon discounting tendencies among those exposed to active defenses; nonetheless Figure 1 shows that the interaction effects observed on the resistance and discounting variables were highly similar.

### *Number of Arguments Listed*

Finally, Table 2 shows that there was a significant main effect of type of defense upon ability to list arguments supporting the truisms. Table 1 indicates that the passive defenses tended to produce greater elevations on this measure than the active defenses. More importantly, however, there was no evidence of the Defense × Time of Attack interaction effect upon this measure that was predicted on the basis of inoculation theory.

On the postexperimental questionnaire each of the experimental subjects was specifically asked, "Do you think an insufficient amount of time was allowed for any section? If yes, list the section." Only 2 of the 72 experimental subjects replied "Yes" to the first question and specifically said they had insufficient time in listing arguments in support of the truisms.

## DISCUSSION

It has already been noted that Papageorgis and McGuire (1961) failed to confirm their prediction that subjects exposed to refutational defenses would be able to list an appreciably greater number of arguments in support of their beliefs in truisms than subjects not exposed to such defenses. Insko (1967) commented that "this measure is crucial for the theory since the resistance supposedly results from the accumulation of belief-bolstering material [p. 302]." The results of the present experiment confirmed McGuire's empirical finding concerning the Defense × Time of Attack interaction effect upon opinion, but failed to provide any support for his interpretation of this effect as a consequence of an accumulation of belief-bolstering material induced by threatening components in active defenses. How can the interaction effect be explained?

One possibility is, of course, that the procedures for assessing the cognitive work following exposure to active defenses were too crude to detect changes in the subjects' cognitive repertories. This alternative continues to have considerable plausibility, though the comparisons of arguments listed by subjects in this experiment and in that of Papageorgis and McGuire (1961) considerably lessen its persuasiveness.

On the other hand, the results do not appear to be readily interpreted in terms of Tannenbaum's congruity formulation. To be sure, the effects upon resistance resembled in many respects the effects upon assertion-weakening tendencies. However, it is not clear how congruity theory can explain the delayed-action effect upon opinion in the active condition.

Perhaps the most tantalizing finding to emerge from the experiment is the suggestion that there may be a tendency, following exposure to active defenses, for delayed attacks to elicit greater discounting than immediate attacks (Figure 1). The orthogonal comparison reported failed to indicate a statistically significant sleeper effect upon discounting tendencies for active defenses. Nonetheless, on each truism there was a tendency for the active defense to produce greater discounting of the delayed attack than of the immediate attack. This tendency can perhaps be explained by supposing that immediately after the active defense the subject feels least confident of his belief in the truism. He has just had the experience of being asked to defend his beliefs against counterarguments, and his poor performance was presumably highly salient for him. It is not surprising, therefore, that an immediate attack under conditions of low self-confidence and low self-esteem does not evoke confident disparagement of the attack. On the other hand, when the subject is exposed to a delayed attack, we may assume that his poor performance in defending his beliefs is no longer highly salient. In short, having regained some measure of confidence and self-esteem during the period following the active defense the subject may feel more confident in minimizing the adequacy of the attacking message. This explanation accounts for the interaction effect in terms of the decaying effects of the threatening components of defensive pretreatments upon the subject's self-evaluations, and on the basis of an assumption that self-evaluations affect the subject's readiness to disparage an attacking message. This account would affirm both the threatening and the assertion-weakening components of defensive pretreatments, but it suggests that they affect resistance to persuasion in a somewhat different manner than proposed by either inoculation or congruity formulations.

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# ATTITUDE CHANGE AS A FUNCTION OF LATITUDES OF ACCEPTANCE AND REJECTION

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Seventy-two subjects, exhibiting identical attitude scales toward the Vietnam war, constructed essays at one of six positions discrepant from their most acceptable position. Resultant attitude shifts were consistent with assimilation-contrast theory, with attitude defined as the total latitude of acceptance. Greatest shifts occurred toward communications two and three positions removed from the end point of the latitude of acceptance.

The relationship between induced attitude change and the discrepancy of a communication from an individual's assessed attitude is an issue of both theoretical and practical importance. Numerous experiments in the area have yielded results that are hard to reconcile, some indicating curvilinear and others linear relationships between increasing discrepancy and attitude change (McGuire, 1966). The Sherif-Hovland (1961) theory predicts more change when the persuasive communication falls in the latitude of acceptance, and some writers like Hovland, Harvey, and Sherif (1957) and Whittaker (1967) write as if attitude change will occur where the communication advocating change falls within an individual's latitude of acceptance. Atkins, Deaux, and Bieri (1967) have presented data in support of the previous interpretation.

The present experiment was designed to determine whether the amount of attitude change exhibited by an individual is predictable from his latitudes of acceptance and rejection and whether there is a curvilinear relationship between attitude change and communication discrepancy as predicted by Whittaker (1967). The present authors test the hypotheses that the relationship between attitude change and communication discrepancy is nonmonotonic; specifically, that the amount of attitude change toward a discrepant communication increases as the position of the communication is displaced from the most accepted position toward the end point of the latitude of acceptance and decreases as the position of the communication is further displaced into the latitude of rejection.

## METHOD

### *Subjects*

Thirty-eight male and 34 female freshmen students were selected as subjects from approximately 1,000 enrolled in introductory psychology classes at Washington State University. Subjects were selected on the basis of their responses to an attitude questionnaire designed to assess their latitudes of acceptance and rejection toward the Vietnam war. All subjects had identical latitudes of acceptance and rejection toward the war and considered the same position most acceptable.

### *Procedure*

The initial questionnaire consisting of 10 items was administered in normal section meetings by the section instructors. In addition to the logical progression of items from one extreme to the other suggested by Sherif, Sherif, and Nebergall (1965), a further check on item selection was instituted by drawing the items from a pool which had previously been given Thurstone rankings by Koulack and Anderson.[2] On the basis of their Thurstone rankings, the items used ran in an orderly progression from a scale value of 1.35 to one of 10.8. The students were instructed to indicate the single item most acceptable to them, the single item most objectionable to them, and any other items which they found acceptable or objectionable (Table 1). Three weeks after the initial administration, each subject was notified of his selection to participate in an experiment to fulfill a course requirement. Subjects reported individually to an office where they were introduced to the experimental task. Each subject was given a standard instruction sheet describing a role-playing task in which the subject was to develop a 500-word essay arguing in support of a particular position toward the Vietnam war. Essay writing has been demonstrated as effective in

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[2] Unpublished study entitled "Attitudes of Washington State University students toward the Vietnam war," 1967.

TABLE 1

Attitude Scale, Initial Position of Subjects

| Statement | | Discrepancy | Mean change —most acceptable position | Mean change —latitude of acceptance | Sample size |
|---|---|---|---|---|---|
| The only solution to the Vietnam war is the complete destruction of North Vietnam. | (0) | | | | |
| The United States should use nuclear weapons in the Vietnam war. | (0) | | | | |
| The United States should risk another round of escalation in Vietnam in order to force Vietnam to seek peace. | (XX) | 0 | | | |
| The United States must stop communist aggression somewhere and South Vietnam is as good a place as any. | (X) | 1 | 0.64 (0.64) | 0.00 (0.00) | 11 (11) |
| Only time will tell whether United States aid to Vietnam will have any value. | (X) | 2 | 0.70 (0.56) | 0.20 (0.11) | 10 (9) |
| The United States should give arms and equipment to the South Vietnamese, but our troops should not be dying there. | (0) | 3 | 0.50 (0.55) | 0.25 (0.27) | 12 (11) |
| The price of war in Vietnam is growing too high to continue. | (0) | 4 | 1.62 (1.82) | 1.31 (1.72) | 13 (11) |
| The United States should stop bombing North Vietnam. | (0) | 5 | 1.50 (1.90) | 1.14 (1.70) | 14 (10) |
| South Vietnam should fight its own war against the communists without United States help. | (0) | 6 | 0.25 (0.50) | 0.33 (0.20) | 12 (10) |
| The United States should withdraw from Vietnam immediately. | (00) | | | | |

Note.—XX, most acceptable position; X, acceptable position; 0, objectionable position; 00, most objectionable position. Data for 62 subjects are indicated in parenthesis.

inducing attitude change by Kelman (1953), Elms and Janis (1965), and Watts (1967), and was used to provide some measure of how much attention subjects directed toward the task.

A second sheet described the particular position each subject was to support and provided several supporting statements which could be used to structure the argument. The position which the subject was to argue was defined by paraphrasing the statement defining the position on the scale. As an example, the fifth discrepancy position on the scale was: "The United States should stop bombing North Vietnam," while the statement given for the subject to argue at that position was: "The United States, for many reasons, should end the bombing of North Vietnam." Supporting statements were based on arguments for a given position found in the popular press at the time. Those supplied for the fifth discrepancy position were: (*a*) "Bombing of civilian populations, whatever the reason, is morally indefensible," (*b*) "Bombing did not destroy the will of Great Britain to continue during World War II," and (*c*) "Bombing creates a climate incompatible with the stated goal of negotiations for peace." Six conditions of communication discrepancy were used in the experimental design, progressing from one position removed through six positions removed from the initial most accepted position. Subjects were randomly assigned to the conditions with male and female subjects approximately balanced. Thus, each subject was required to develop an essay of 500 words supporting a position at some discrepancy from his or her own most acceptable position. Subjects returned their essays 48 hours later. At that time, the questionnaire was readministered, and the experimental manipulation was explained.

## Results and Discussion

A scoring manual developed prior to the experiment used such measures as number of arguments, number of citations, length, and overall quality to assess the essays. Ten essays spread across five of the six discrepancy conditions were judged not to be comparable to the other essays in terms of the criteria. Rather than completely eliminate 14% of the subjects particularly with a somewhat higher concentration of subjects at Position 5, an analysis was made on both the original 72-subject group as well as a 62-subject group. Changes in scale position from pre- to post-communication measures were analyzed in terms of shifts of the most acceptable position and by extension or retraction of the latitude of acceptance relative to the most objectionable position. Mean change scores over all six conditions of the most acceptable position, although in the predicted direction, were not statistically significant for either the 72-subject ($F = 1.76$, $df = 5/66$) or 62-subject ($F$

TABLE 2

TREND ANALYSIS, EXTENSION OF LATITUDE OF ACCEPTANCE

| Source | SS | df | F |
|---|---|---|---|
| Linear trend | 4.99 | 1 | 4.16* |
| Deviation from linear trend | 13.51 | | |
| Quadratic Trend | 6.06 | 1 | 5.06* |
| Deviation from quadratic trend | 7.46 | | |
| Cubic Trend | 5.63 | 1 | 4.70* |
| Deviation from cubic trend | 1.83 | | |
| Quartic Trend | 0.13 | 1 | 0.11 |
| Deviation from quartic trend | 1.70 | | |
| Higher order components | 1.70 | 1 | 1.42 |

* $p < .05$.

$= 2.01$, $df = 5/56$) group. Mean change scores for the extension of the latitude of acceptance toward the most objectionable position were statistically significant for both the group of 72 ($F = 3.09$, $df = 5/66$, $p < .01$) and 62 ($F = 7.56$, $df = 5/56$, $p < .001$) subjects. A trend analysis (Gaito, 1965) based on the extension of the latitude of acceptance used the data for all 72 subjects (Table 2). Although the interval assumptions of the technique are not completely applicable to the present scale, the effects do demonstrate significant nonmonotonic trends consistent with the hypothesis.

The results are in partial agreement with the assimilation-contrast paradigm advanced by Sherif and Hovland (1961). The most persuasive messages are those in Positions 4 and 5, slightly within the latitude of rejection, while those at the extreme discrepancy of Position 6 produce decidedly less change. The results of the present experiment also suggest a different interpretation of the data presented by Atkins et al. (1967), proporting to show maximum change occurring within the latitude of acceptance. In their design, subjects scaled the communication after it was presented. A shift of the scale toward the anchor stimulus as predicted by Sherif and Hovland (1961) could have occurred prior to the subjects' scaling of the communication, leading them to perceive the stimulus as being within their latitude of acceptance when initially it was not. In the present study, subjects at Positions 4 and 5 came to perceive the communication as lying within their latitudes of acceptance, although initially the positions were well into their latitudes of rejection. Thus, attitude change is most likely to occur in response to a communication when its placement is a short distance removed from the end point of the latitude of acceptance. The subject then will react to the anchor stimulus by extending his latitude to encompass it. Further investigation is necessary to determine whether maximum change occurs at the same position relative to the latitudes of acceptance and rejection across other scales.

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# RELATIONSHIP BETWEEN THE QUALITY OF COUNTER-ATTITUDINAL PERFORMANCE AND ATTITUDE CHANGE[1]

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Male subjects composed and delivered short counterattitudinal speeches opposing draft deferments for college students. Relationships between the quality of counterattitudinal performance and amount of pro-speech attitude change were found to depend upon (*a*) whether performance was evaluated by judges or by the subjects themselves, (*b*) whether the audience was believed to agree or disagree with the position being advocated, and (*c*) whether or not the audience was thought to realize the subject was role playing. Thus subjects' impressions of their audience were important mediators of the relationship under investigation. The findings are not readily assimilated to any existing theory of counterattitudinal advocacy.

A rapidly expanding body of literature (reviewed by Elms, 1967) suggests that people who are induced to compose and deliver counterattitudinal messages tend to bring their attitudes into accord with the communications they produce. But investigators (Carlsmith, Collins, & Helmreich, 1966; Festinger & Carlsmith, 1959; Greenbaum, 1966; Zimbardo, 1965) have reported little relationship between the quality of counterattitudinal messages and the amount of attitude change. Elms (1967) has suggested that the performance measures employed by these researchers may have been too crude to disclose expected relationships. This paper explores other possible explanations for weak and inconsistent findings.

In most of the pertinent studies, performance has been evaluated by judges who have examined the length, cogency, or logical consistency of subjects' counterattitudinal messages. But the theories that have guided the research suggest that subjects' appraisals of their own performance are more appropriate measures of quality. Dissonance theory (Festinger, 1957; Festinger & Carlsmith, 1959) contends that it is the inconsistency between *perceived* performance and belief that is critical, and self-persuasion theory (Janis & Gilmore, 1965; Janis & King, 1954) asserts that attitude change occurs because the subject is impressed by the quality of his own arguments. When applied to certain settings, incentive theory (Hovland, Janis, & Kelley, 1953; Linder, Cooper, & Jones, 1967) seems to imply that the subjects' personal evaluations are especially important. Thus a subject who believes he has performed well in his assigned role should expect more approval from the experimenter than should a subject who believes he has performed poorly. Under circumstances to be described below, a subject who believes he has performed well may also expect more approval from his audience.

If "objective" measures of performance were known to be highly correlated with subjects' own appraisals, the use of the former to test propositions involving the latter would pose no problem. But Zimbardo (1965) found virtually no relationship between judges' ratings and subjects' evaluations of their own counterattitudinal behaviors. The present study employs both kinds of indexes.

When an experimental subject produces a counterattitudinal message, he ordinarily addresses it to someone other than the experimenter who instructed him to produce it. Investigators have generally neglected this audience, choosing instead to concentrate their attention upon the amount and kind of inducement offered the subject, and the subject's freedom to decline to participate. This paper examines two aspects of the subject's relationship to his audience.

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A subject may believe that his audience realizes he is role playing, or he may believe that the audience assumes he is behaving freely. The former impression should prevail when the subject is *publicly assigned* his task, and the latter should be created when the subject is *privately assigned*. In most studies of counterattitudinal advocacy assignment has been private. Consequently, subjects have probably expected their audiences to regard them as free agents.

A subject may believe that his audience *does* or *does not* approve the view he is defending. Some of the research by dissonance theorists appears to have involved messages that were counterattitudinal for the audience as well as for the communicator, but it is generally unclear whether subjects believed their audiences would agree or disagree with the position they were induced to advocate.

In the great majority of studies subjects have received little or no evaluative feedback from their audience. The few investigators who have provided clear feedback (Scott, 1959; Wallace, 1966) have reported that audience approval leads to more attitude change than does audience disapproval. Consequently, it is reasonable to predict that experimental conditions that induce subjects to *believe* a strong counterattitudinal performance will evoke strong audience approval will generate positive correlations between subjectively evaluated performance and attitude change. Conversely, conditions that induce subjects to believe a strong counterattitudinal performance will evoke audience disapproval should generate negative correlations between subjective performance and attitude change. Whether subjects will expect a strong performance to yield approval or disapproval should depend upon whether they believe their message is counterattitudinal *for the audience*, and upon whether they believe their audience assumes they are behaving freely.

A privately assigned subject who believes he has performed well before an audience that disagrees with the position he has advocated should anticipate audience rejection because the audience will believe he genuinely espouses the position he has defended so vigorously. But a publicly assigned subject should expect a disagreeing audience to realize that his strong performance is not necessarily indicative of personal commitment. Consequently, a publicly assigned subject may expect approval for performing skillfully, and need not anticipate rejection for attacking the preferences of his audience. Thus quality of counterattitudinal performance (as judged by the subject himself) should be negatively correlated with perceived audience approval (and with attitude change) when a privately assigned subject addresses a disagreeing audience; quality of performance should be positively correlated with perceived audience approval (and with attitude change) when a publicly assigned subject addresses a disagreeing audience.

The effect of public versus private assignment should be much less pronounced when the audience is believed to agree with the position being advocatd in the counterattitudinal message. Regardless of the character of the assignment, the stronger a subject believes his performance has been, the more approval he should expect from an agreeing audience. However, because private assignment presumably implies greater personal commitment than does public assignment, the former should probably be expected to produce somewhat stronger relationships between subjectively evaluated performance and perceived audience approval.

This paper reports a study in which subjects are publicly *or* privately assigned responsibility for delivering a short counterattitudinal speech before an audience that is believed to agree *or* disagree with the advocated posiiton. As noted above, a theory that stresses the reinforcing effects of perceived audience approval predicts that subjectively evaluated performance will be negatively correlated with attitude change when a privately assigned subject addresses a disagreeing audience. The theory predicts moderately positive correlations in the other three conditions.

A literal interpretation of self-persuasion theory suggests that perceived audience approval is irrelevant to attitude change, and that correlations between self-evaluated performance and attitude change will be positive in all four experimental conditions. An incentive theory that stresses the perceived approval of the experimenter as the critical reinforcer will make the same predictions as does self-persuasion theory.

It is difficult to derive confident predictions from dissonance theory. To be sure, a strong subjective performance should be more inconsistent with the subject's initial attitude than a weak performance. But a strong performance may also lead the subject to expect strong experimenter and/or audience approval ("consonant cognitions" that justify the strong performance). In the absence of criteria for specifying whether the latter consequences of a persuasive performance will negate the former, dissonance theory is equivocal concerning relationships between quality of counterattitudinal behavior and amount of attitude change. However, it should be noted that a strong performance by a privately assigned subject who addresses a disagreeing audience should generate less perceived audience approval than a strong performance in any of the other three conditions. Therefore, a strong performance in the former condition should be more likely to arouse dissonance, and provoke attitude change, than should a strong performance in any of the other three experimental treatments. According to this line of reasoning the most highly positive correlation between subjectively evaluated performance and attitude change should occur when privately assigned subjects address a disagreeing audience. This prediction is directly contrary to that derived from the theory of perceived audience approval.

In addition to examining relationships between objective and subjective indexes of performance, and between each type of performance measure and attitude change, this study explores possible effects of the experimental manipulations on the performance measures. It is conceivable that the subject's conception of his audience affects his actual performance, or his perception of his performance.

## METHOD

### *Subjects*

The subjects were 83 male students from the introductory psychology course at the University of Illinois. They received research credit for participation in the study.

### *First Session*

Subjects were assembled in groups of approximately 20 persons, and responded to a series of questionnaires. The only instrument that was pertinent to the present study was a six-item "opinion survey." The questions in this instrument called for expressions of opinion concerning issues such as legalizing the use of marijuana, reducing the voting age, and permitting Communists to speak on the campus. All questions were answered by checking nine-step graphic scales, the ends of which were labeled "approve very strongly" and "disapprove very strongly." The only item that was subsequently scored concerned continuation of draft deferments for college students. In response to this question all but two of the subjects checked one of the three spaces at the "strongly approve" end of the scale. The two subjects who reported more moderate views were not scheduled for the second session.

### *Second Session*

The second session was held 4–17 days after the first. Subjects reported singly to a small research room where they were met by an experimenter who had not been present at the first session. (The two sessions were held in different rooms and two separate postcards were used to notify subjects of their appointments. No subject reported that he saw any connection between the two sessions.) The experimenter explained that he was cooperating with the speech department in a study of persuasive communication. The research was said to involve tape-recording a number of short speeches which would be played before an audience of 20 or 30 townspeople. Members of the audience would be asked to rate the content and delivery of the speeches they heard, and would provide the data of the research. The issue of "draft deferments for college students" was said to have been chosen as the topic for all speeches because it was a matter of wide public interest. The experimenter then asserted that he had already collected enough speeches favoring the continuation of draft deferments but would like the student to make a speech opposing them. Only one subject protested this assignment, and he was excused from the study. Each of the remaining 80 subjects was told that he would have 5 minutes in which to compose his speech, and would be permitted to talk for as long as 6 minutes. At this point instructions were varied to create four experimental groups.

*Public role assignment.* Forty randomly selected subjects were told that the experimenter would introduce them by name and would announce that they had arbitrarily been assigned to speak against the continuation of draft deferments for college students.

*Private role assignment.* The remaining 40 subjects were told that the experimenter would introduce them by name and would announce that they were going to express their views on the issue of draft deferments.

*Disagreeing audience.* Half of the subjects in each of the above categories were told that previous work with the particular audience that would hear the speeches indicated that about 90% of these people supported draft deferments for college students.

*Agreeing audience.* The remaining subjects in each category were told that previous work with the audience that would hear the speeches indictated that about 70% of these people opposed draft deferments for college students. (It would have been desirable to represent both the agreeing and disagreeing audiences as being 90% unanimous. However, it appeared likely that an audience 90% opposed to draft deferments for college students would seem unbelievable to subjects.)

After 5 minutes of preparation time the experimenter turned on the tape recorder and introduced the speaker in the manner described above. Three subjects who spoke for more than 6 minutes were halted less than a minute after the alloted time had elapsed. The length of each subject's speech was recorded in seconds.

After completing his speech, the subject indicated how he actually felt about draft deferments by checking a graphic scale that was an exact duplicate of the one employed during the first session. He also responded to five scales designed to reveal his assessment of his performance. These items called for a judgment of the convincingness with which he thought he had spoken, and for estimates of the ratings the audience would give him on persuasiveness, sincerity, poise, and command of language. Other items in the questionnaire asked the subject to report the degree of comfort he had felt while speaking, and to estimate how much the *typical* member of the audience would like to have him as a personal friend. All items in the questionnaire were answered by checking nine-step graphic scales.

*Debriefing.* Subjects were questioned concerning their impressions of the study and were given a brief but truthful explanation of its purpose. They were assured that their speech would not be played before any audience.

### Measures

*Attitude change.* On two different occasions subjects reported their attitudes toward continuation of draft deferments for college students. Their responses to the nine-step graphic scales were quantified by assigning values ranging from 1 (disapprove very strongly) to 9 (approve very strongly). Subjects' attitude-change scores were obtained by subtracting their postspeech scores from their prespeech scores. Large positive attitude-change scores indicate large changes *toward* the position advocated in the speech (i.e., opposition to deferments).

*Subjective appraisal of performance.* The subject's responses to the five items dealing with the quality of his performance were assigned values ranging from 1 (least favorable) to 9 (most favorable). Correlations among responses to the five items ranged from .39 to .79. In view of the magnitude of these interrelationships, a single score was obtained by summing responses across all five items. (Separate analyses based on single items were found to yield very similar results and will not be reported here.)

*Other measures.* Responses to the scales dealing with the comfort experienced by the subject and the audience's desire to have the subject as a friend were each assigned values ranging from 1 (least comfort, least desire as a friend) to 9.

*Judge's ratings.* After all speeches had been tape-recorded, they were evaluated by a judge who was unfamiliar with the nature of the study. The introductory statements that identified the speakers as having been assigned, or not having been assigned, to speak against draft deferments were removed from the tapes before they were evaluated. The judge rated the subject's performance on four five-step scales: poise of speaker, content of speech, number of different arguments, and overall quality of performance. Since correlations of the first three of these ratings with overall quality ranged from .64 to .86, only the judge's ratings of overall quality will be employed in the discussion that follows. Separate analyses for the other three ratings were conducted and found to yield very similar results.

A second judge independently rated the overall quality of 31 of the speeches. The correlations between the ratings of the two judges was .57.

*Length of speech.* Subjects were permitted to cease speaking whenever they had expressed all their arguments in favor of discontinuing draft deferments. The lengths of speeches ranged from 35 to 410 seconds.

## ANALYSIS AND RESULTS

### Effects of Manipulations on Means and Standard Deviations of Scores

Table 1 reports the means and standard deviations of the scores earned by the four experimental groups and by the total sample of 80 subjects. Fixed-effects analyses of variance revealed no significant main or interaction effects of the experimental manipulations of any of the variables. However, the effect of private versus public assignment on attitude change reached the .10 level ($F = 3.53$, $df = 1/76$). In view of the marginal characted of this finding, the effect of assignment on attitude change was examined further through the use of covariance analyses. When length of speech was employed as the covariate the effect of assignment reached the .06 level of significance, but the use of judge's ratings and self-ratings as the covariate did not appreciably affect outcomes.

It is to be noted that the standard deviations of scores do not vary much from one

TABLE 1

MEANS AND STANDARD DEVIATIONS OF SCORES EARNED BY FOUR CATEGORIES OF SUBJECTS

| Measure | Assignment and audience position | | | | All *Ss* |
|---|---|---|---|---|---|
| | Private-Agree | Private-Disagree | Public-Agree | Public-Disagree | |
| Length of speech | | | | | |
| *M* | 165.5 | 152.2 | 129.9 | 172.0 | 154.9 |
| *SD* | 112.7 | 70.1 | 56.5 | 83.8 | 83.4 |
| Judge's rating | | | | | |
| *M* | 2.3 | 2.4 | 2.6 | 2.4 | 2.4 |
| *SD* | .9 | 1.0 | 1.0 | .8 | .9 |
| Self-rating | | | | | |
| *M* | 20.0 | 18.8 | 20.9 | 19.1 | 19.7 |
| *SD* | 7.0 | 5.4 | 5.9 | 7.1 | 6.3 |
| Attitude change | | | | | |
| *M* | .7 | .5 | 1.6 | .9 | .9 |
| *SD* | 1.6 | 1.1 | 1.5 | 1.9 | 1.6 |
| Comfort | | | | | |
| *M* | 3.4 | 3.5 | 3.6 | 3.0 | 3.4 |
| *SD* | 1.8 | 1.4 | 1.5 | 1.4 | 1.5 |
| Audience friendliness | | | | | |
| *M* | 4.9 | 5.0 | 5.1 | 5.3 | 5.1 |
| *SD* | 1.7 | 1.0 | 1.3 | 1.3 | 1.3 |

treatment condition to another. When evaluated by *F* test, only 1 of 36 pairs of standard deviations yielded a significant difference (private-agree > public-agree on length of speech; $p < .05$).

*Correlations among Nonattitudinal Measures*

Although the previous analysis indicates that the experimental manipulations had little or no effect on the means and standard deviations of scores, possible effects on the *interrelationships* among scores remain to be examined. Tables 2 and 3 report the pertinent data.

Table 2 reveals little overall impact of the manipulations on relationships among nonattitudinal measures. Although the correlation coefficients for some pairs of variables fluctuate markedly from one experimental group to another, only the data for the relationship between self-ratings and audience friendliness achieve statistical significance. As postulated by audience-approval theory, self-ratings are negatively correlated with perceived audience friendliness in the private-disagree treatment, but positively correlated with audience friendliness in each of the other three groups. The difference between the private-disagree treatment and *each* of the other groups is significant ($p < .05$).

TABLE 2

CORRELATIONS AMONG NONATTITUDINAL MEASURES

| Correlated variable | Assignment and audience position | | | | All *Ss* |
|---|---|---|---|---|---|
| | Private-Agree | Private-Disagree | Public-Agree | Public-Disagree | |
| Length of speech—judge's rating | .31 | .18 | .51* | .51* | .39** |
| Length of speech—self-rating | .20 | .32 | .53* | .24 | .33** |
| Judge's rating—self-rating | .53* | .06 | .32 | .09 | .26* |
| Length of speech—comfort | .36 | .21 | .42 | .43 | .36** |
| Judge's rating—comfort | .22 | −.09 | .39 | .15 | .17 |
| Self-rating—comfort | .59** | .55* | .69** | .77** | .66** |
| Length of speech—audience friendliness | −.03 | .02 | −.12 | .13 | .00 |
| Judge's rating—audience friendliness | .41 | .07 | .00 | −.08 | .12 |
| Self-rating—audience friendliness | .54* | −.18 | .50* | .69** | .43** |

* $p < .05$.
** $p < .01$.

For the entire sample the correlation between the two objective measures of performance was .39 ($p < .01$), and the correlations between self-ratings and the two objective measures were .33 ($p < .01$) and .26 ($p < .05$). These meager relationships suggest that the three performance indexes are unreliable measures and/or that they are measuring different phenomena. The first of these possibilities seems an inadequate explanation of the low intercorrelations. The procedure by which length of speech was recorded allowed little opportunity for measurement error to occur, and the interjudge reliability of the judge's ratings was .57. Furthermore, self-ratings correlated substantially higher with other variables (e.g., comfort) than with the two objective indexes, suggesting that errors of measurement did not impose unduly severe restrictions on the magnitudes of possible correlations. In view of these facts, it seems appropriate to agree with Zimbardo's (1965) suggestion that objective and subjective performance measures reflect different phenomena.

It is noteworthy that self-ratings tended to correlate much more strongly with rated comfort and perceived audience friendliness than did either of the objective measures.

### *Correlation of Other Variables with Attitude Change*

Table 3 indicates that, for the entire sample, none of the nonattitudinal variables was significantly related to attitude change. However, it is apparent that the experimental manipulations had substantial effects on the obtained correlations. Length of speech and judge's ratings were significantly more positively related to attitude change when subjects addressed an agreeing audience than when they spoke to a disagreeing audience ($p < .02$ in each case). Self-ratings and reported comfort were more positively related to attitude change when assignment was public than when it was private ($p < .01$ in each case). Thus the correlations of four (of the five) nonattitudinal variables with attitude change revealed a significant "main effect" of the experimental manipulations.[3] Two significant "interaction effects" were also obtained. When the audience was said to disagree( but not when it was said to agree), public assignment produced stronger positive relationships between audience friendliness and attitude change than did private assignment. When the audience disagreed the effect of private versus public assignment on correlations between self-ratings and attitude change was significantly greater than was the case when the audience agreed. The latter interaction is consistent with audience-approval theory which contends that subjective performance will be negatively correlated with attitude change when a privately assigned subject addresses a disagreeing audience (obtained $r = -.44$), and positively correlated with attitude change when a publicly assigned subject addresses a disagreeing audience (obtained $r = .60$). When the audience agreed, the obtained correlations between subjective performance and attitude change were, as anticipated, relatively unaffected by private versus public assignment, but these correlations (.04 and .12) were less positive than the theory would predict.

In further analyses, correlations were computed between specific performance measures and attitude change, with the variance attributable to other performance measures partialed out. However, the weak interrelationships among performance measures dic-

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[3] In testing the main effects of the manipulations, the correlation coefficients for a specific relationship (e.g., self-ratings—attitude change) were entered in a four-cell table, the columns of which represented two degrees of audience agreement, while the rows represented two types of assignment. The two $z$-score equivalents in the "disagree" column were summed, as were the two $z$-score equivalents in the "agree" column. The difference between the two sums obtained in this fashion was entered as the numerator of a ratio in which the denominator was $\sqrt{\frac{4}{n-3}}$. (The denominator is the standard error of the difference between pairs of values each of which is the sum of two $z$ scores for independent samples of size $n$. The obtained ratio equals the normal deviate of the difference between two such values.) Parallel operations were performed on the rows of the table to evaluate the effect of assignment. The interaction effect of assignment and audience agreement was tested by subjecting the diagonals of the table to the same kind of analysis. The authors are indebted to Ledyard R. Tucker for guidance in the use of these statistical procedures.

TABLE 3

Correlations of Five Variables with Attitude Change

| Measure | Assignment |  | Audience |  | Assignment and audience position |  |  |  | All $Ss$ |
|---|---|---|---|---|---|---|---|---|---|
|  | Private | Public | Agree | Disagree | Private-Agree | Private-Disagree | Public-Agree | Public-Disagree |  |
| Length of speech | .12 | .34* | .59** | −.20 | .59** | −.42 | .59** | .03 | .23 |
| Judge's ratings | .12 | .24 | .38* | −.09 | .23 | .00 | .59** | −.19 | .18 |
| Self-ratings | −.20 | .39* | .08 | .11 | .04 | −.44 | .12 | .60** | .08 |
| Comfort | −.10 | .49* | .27 | .15 | .08 | −.27 | .44 | .53* | .21 |
| Audience friendliness | .00 | .10 | .04 | .06 | .29 | −.29 | −.21 | .39 | .05 |

* $p < .05$.
** $p < .01$.

tated that the results of these analyses would not be greatly different from those obtained using bivariate techniques. The overall pattern of relationships between self-ratings and attitude change was not weakened when effects attributable to objective performance were partialed out, but the two correlations for the "audience agree" treatments changed from weak positive to weak negative. Similarly, patterns of relationships between objective measures and attitude change were not greatly affected by removal of variance attributable to subjective performance.

## Discussion

The experimental manipulations did not have large effects on the means or standard deviations of the performance measures, and their impact on mean attitude change scores was only marginally significant. But the manipulations had substantial effects on relationships between performance measures and attitude change. No existing theory of counterattitudinal advocacy appears to receive strong or consistent support from the findings.

According to the theory of audience approval advanced earlier in this paper, a high subjective appraisal of one's performance should create an expectation of audience *disapproval* when a privately assigned subject addresses a disagreeing audience, but an expectation of audience approval in the other three treatments. Obtained correlations between self-ratings and ratings of audience friendliness are consistent with this contention. Also consistent with audience-approval theory is the striking effect of private versus public assignment on correlations between the self-ratings and attitude-change scores of subjects who addressed a disagreeing audience, and the absence of such an effect when the audience was presumed to agree. But correlations between the self-ratings and attitude-change scores of those who addressed an agreeing audience are less positive than audience approval theory would predict. Perhaps the approval of an audience already committed to the view the subject is advocating represents so little reward that it cannot have large effects on attitude change. Equally likely is the possibility that subjects do not value the approval of an "agreeing audience" when it constitutes an outgroup, as in the present study.

More damaging to the case than can be made for audience approval theory is the fact that correlations between perceived ratings of audience friendliness and attitude change are low for all treatment groups. Perhaps ratings of audience friendliness are not, as was assumed when the study was designed, a good measure of perceived audience approval. But, in the absence of evidence on this point, one must maintain serious doubts concerning the validity and/or completeness of audience approval theory.

Alternative formulations appear to fare no better than audience approval theory. Explanations that stress the importance of self-persuasion or experimenter approval should predict strong positive correlations between self-ratings and attitude change in all four treatment groups. Such a prediction receives virtually no support from the data.

As noted earlier, dissonance theory does not yield unequivocal predictions concerning the outcomes of this study. Self-ratings should presumably correlate positively with dissonance arousal (and with attitude change) provided a strong performance does not generate "consonant cognitions (e.g., perceptions of strong audience or experimenter approval). No data are available concerning perceived experimenter approval, but the evidence suggests that the private-disagree treatment permitted subjects to experience less audience approval for a strong performance than did any of the other treatments. Consequently, self-ratings should have been most positively related to attitude change in the private-disagree group. The obtained results are directly contrary to this derivation.

The findings are also contrary to other derivations from dissonance theory. According to Carlsmith, Collins, and Helmreich (1966) and Linder, Cooper, and Jones (1967), subjects who are induced to deceive an audience concerning their true beliefs should experience more dissonance than subjects whose performance does not involve this deception. Thus privately assigned subjects should experience greater dissonance than those who are publicly assigned, and, according to the usual assumption of dissonance theory (high dissonance produces more attitude change than low dissonance), should manifest larger attitude changes. The findings, though not statistically significant ($p < .10$), are opposite to the prediction. Furthermore, no support is evidenced for the contention that deceiving someone like oneself generates more dissonance than does deceiving a less similar person (Linder et al., 1967). Privately assigned subjects who deceived an audience that shared their true opinions concerning draft deferments showed slightly less attitude change than did those who deceived a person with whom they were in genuine disagreement.

Subjects' ratings of their comfort were obtained as possible indicants of dissonance arousal. As reported in Table 2, these ratings were highly correlated with subjective performance in all treatment groups, suggesting that they do not reflect the intensity of dissonance arousal *or* that a strong subjective performance does not arouse more dissonance than a weak one. The fact that comfort ratings are significantly more positively related to attitude change when subjects are publicly, rather than privately, assigned seems not to be derivable from any of the theories mentioned above.

All major theories of counterattitudinal advocacy imply that the objective quality of performance influences attitudes only if it is associated with subjective performance or with the individual's impression of the costs and rewards connected with his behavior. But the data indicate that objective performance is significantly related to attitude change when the audience is presumed to agree, whereas subjective performance is not. Furthermore, this relationship between objective performance and attitude change is not subtantially reduced when the variance attributable to comfort and perceived friendliness is partialed out. Perhaps there are other perceived costs and rewards (e.g., effort, responsibility, experimenter approval, satisfaction over having deceived an outgroup) that will explain the obtained results. The data of this study provide considerable information concerning what is *not* happening when an objectively strong performance produces large attitude change; they provide few clues as to what *is* happening.

It is clear that the subject's impressions of his audience influenced relationships between quality of performance and attitude change. When quality was assessed by judge's ratings, or by measuring the length of the speech, relationships to attitude change were significantly affected by the subject's assumption that the audience agreed or disagreed with his arguments. When the quality was evaluated by the subjects themselves, its relationship to attitude change depended both upon perceived audience agreement and upon whether the audience was believed to realize the subject was role playing. Audience-approval theory provides only a partial explanation of these findings, and other theories of counterattitudinal behavior fail to postdict these outcomes. Whatever the correct explanation may be, the data suggest that the subject's impression of his audience is an important element in the situation. Perhaps increased concern for this generally neglected aspect of counterattitudinal advocacy will identify reasons for the weak and contradictory findings that characterize this area of inquiry.

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# ON INFERRING ONE'S BELIEFS FROM ONE'S BEHAVIOR [1]

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Subjects were led to believe that they were going to deliver arguments against air pollution to passers-by in the street. At the same time, confederates were asked to argue for promoting auto safety. The confederate stated that he was willing to argue in favor of auto safety because (*a*) he believed strongly in it (belief-relevant condition), or (*b*) the experiment was scientifically valuable (belief-irrelevant condition). Belief-relevant subjects were found to be more opposed to air pollution than either belief-irrelevant subjects or control subjects who were not committed to the behavior. The results support Bem's hypothesis that people infer their beliefs from their behavior and suggest that such a process occurs only when there are cues implying that behavior is relevant to belief.

Bem (1965, 1967) has proposed that people infer their beliefs, to some degree, from their behavior. He suggested that in assessing their attitudes, people may perform a sort of behavioral inventory, asking themselves what their past behavior seems to imply about their attitudes. Thus, when an individual is asked if he likes brown bread, he may respond with, "I guess I do, I'm always eating it."

Despite the plausibility and potential importance of Bem's notion, there is little evidence that one can point to in support of it. The idea was originally introduced in the context of what has become a major controversy concerning the interpretation of dissonance phenomena. Bem believes that the basic findings of both the "forced compliance" type of dissonance experiment (e.g., Festinger & Carlsmith, 1959) and the "free choice" type (e.g., Brehm, 1956) can be explained without recourse to the motivational interpretation preferred by dissonance theorists. In forced compliance experiments, for example, subjects are typically induced to behave in a way which is inconsistent with the beliefs—with the consequence that their beliefs change to become more consistent with their behavior. The dissonance theorist's view is that such subjects find the belief-behavior discrepancy aversive and change their beliefs in order to justify their behavior. Bem's view is that such subjects are merely observing their behavior and inferring their beliefs from it: "If I behaved in that way, then my beliefs must be consistent with the behavior."

While it is possible to explain many dissonance experiments with the notion that people infer their beliefs from their behavior, such experiments cannot be regarded as a test of the notion. Moreover, it is difficult to conceive of an experiment involving counterattitudinal behavior that could provide unambiguous support for Bem's notion. The dissonance theorist will be able to assert in such cases that the counterattitudinal behavior produced dissonance, hence attitude change in the direction of the behavior.

There is some evidence bearing on the hypothesis from experiments where subjects were asked to behave in a manner consistent with their beliefs—a situation in which dissonance explanations cannot be very persuasively advanced. The most convincing such experiment is one by Bandler, Madaras, and Bem (1968), where subjects were asked to press a button in order to escape electric shock. Subjects were told, in effect, that if they found the shock not at all uncomfortable they need not escape it, but the experimenter would really prefer it if they did. Under such conditions subjects rate the shocks as being more painful than when they are not allowed to escape the shocks at all. The authors interpret this finding to mean that subjects inferred that the shocks were especially painful when they "chose" to escape them.

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The previous experiment gives some support to the notion that people infer their attitudes from their behavior. However, work by Kiesler and his colleagues on consonant behavior (Kiesler, Pallak, & Kanouse, 1968; Kiesler & Sakumura, 1966) could also be construed as a test of the notion and seems, at least on the surface, to contradict it. In both studies subjects in control conditions were induced to read a speech consistent with their beliefs into a tape recorder. They were told that the recordings would be used in a study of regional speech accents. Simply recording the speech had no effect on subjects' descriptions of their attitudes, apparently contradicting the notion that subjects infer their attitudes from their behavior. This conclusion can be avoided, however, if we assume that subjects infer their attitudes from their behavior only when there is some kind of implication, supplied by the culture or by situational cues, that the behavior in question is relevant to their beliefs. Kiesler and his colleagues tried to make the consistent behavior seem very inconsequential (cf. Kiesler, 1968), and, as a result, their subjects probably did not think that reading a speech for "a study of regional speech accents" was particularly relevant to their views on the topic they spoke about in the recording.

Thus, the discrepancy between the work of the Bem group indicating that people infer their beliefs from their behavior and work by the Kiesler group indicating that they do not may only be an apparent one. Escape behavior probably carries a strong implication that the stimulus escaped is noxious. Speech-reading behavior, on the other hand, does not necessarily imply adherence to the contents of the speech. It may be that many behaviors are of this latter neutral sort, suggesting nothing about belief except when situational cues intervene with an implication that the behavior is relevant to belief.

Such reasoning suggests an experiment in which subjects are asked to carry out some consonant behavior which does not in itself imply anything about belief. Some of the subjects should be led to think that the behavior does indeed have implications for belief, while the implication should not be drawn for other subjects. The subjects who assume that a link exists between behavior and belief should express greater agreement with the behavior-consonant beliefs than subjects who do not assume that such a link exists.

## METHOD

### *Overview*

Subjects were asked to be "experimenters" in a study allegedly designed to determine the optimal number of arguments to use in a persuasive communication. They were to point out the importance of combatting air pollution to passers-by in the street and urge them to sign an anti-air-pollution petition. A confederate was asked to deliver similar arguments about the importance of promoting auto safety. When the experimenter asked subject and confederate if they were agreeable to presenting arguments on their assigned topics, the confederate delivered the experimental manipulation, saying either that he was glad to promote a cause he believed in or that he was pleased to participate in an experiment that might have some scientific value. The confederate thus served as a model for the subject: in the one case he implied that the sort of behavior to which the subject was committed was relevant to belief, and in the other case he drew no such implication. The experimenter then asked subject and confederate, before leaving to carry out their tasks, to respond to several opinion items, including items assessing attitudes toward air pollution.

In order to determine whether commitment to the behavior produced attitude change by itself, in the absence of a cue linking behavior to belief, a control condition was included in the design. The control condition was composed of subjects who merely observed the experimental situation and did not expect to perform the behavior.

### *Subjects*

Sixty-one Yale freshmen who had never taken a psychology course were recruited to "serve as experimenters in an interesting study on communication and persuasion." Each subject was paid $1.50 immediately upon arrival. Three experimental subjects indicated they were suspicious (one to the experimenter and two to the confederate), and they were discarded from the analysis.

### *Procedure*

Two subjects were invited to each experimental session. Usually both subjects showed up, and one was randomly assigned to be a participant, while the other was assigned to the witness condition. In half the cases, the first subject who arrived was assigned to be the witness; in the other half, the second subject was the witness. However, a rigged drawing was held so that the witness did not feel that he was arbitrarily selected for the task. When only one subject showed up for the experiment, he was assigned to the participant condition, and the scenario was changed to delete the few sentences referring to the witness' role.

The experimenter began by telling the subjects, and a confederate whose arrival was timed to match that of the subjects, that he was "conducting two different studies at the same time" and that "one of you will take part in one study" and "two of you will take part in another study." After the rigged drawing, the experimenter introduced both studies as part of a project examining the relationship between the number of arguments used in a communication and the persuasiveness of the communication:

> The analogy I like to think of is whether a commercial—say on TV—with two arguments will make me more or less willing to buy the product than a commercial with three arguments, and so on. Actually, my hypothesis is that the more arguments you have, the greater agreement you'll get, but only up to a certain number. . . . The strategy of my project is twofold. First, I have to find reliable and valid materials for several opinion topics. Second, using the materials, I have to give different subjects a different number of arguments in favor of a particular opinion topic and simply find out what the relationship is between the number of arguments given and the agreement with the opinion.

### *Commitment to the Behavior*

To create the participant conditions, the experimenter turned to the confederate and the participant and explained the nature of their task:

> Now, Mr. [Confederate] and Mr. [Participant], you two are going to be my experimenters and actually run some subjects. What this means is that you will present some arguments, which I've already prepared, to four or five people, time permitting, on the corner of College and Chapel [two of New Haven's main streets] and then find out if they agree with the opinion. The reason I need to have students act as experimenters is that I can't allow the specific personality characteristics of a single experimenter to affect the results. The more experimenters I have, the more certain I'll be that the results will not be due to some idiosyncratic personality style . . . . The arguments themselves are very simple, straightforward facts that I've already prepared—so you won't have to improvise on the spot. After you present the arguments, you'll ask the person whether or not he is willing to sign a petition in favor of your opinion. I'll give you all the material in just a minute and you can rehearse and practice on me before you leave . . . . You won't be allowed to deviate in any way from the procedure.

Thus, the participant was led to expect that he would be leaving the experimental room to persuade people on the streets of New Haven about some opinion topic.

To create the witness condition, the experimenter turned to the witness and explained the nature of his task:

> Mr. [Witness], you won't be going out on the streets. You're going to work on the reliability and validity problem of a new topic I might use in the near future . . . . You'll be judging the quality of the arguments on the topic of maritime safety . . . . While they're [the confederate and the participant] rehearsing, we'll go to the other room and I'll get you started.

Thus, the witness was led to expect that he would be staying in the experimental room to work on materials for an unrelated attitude topic. However, he saw and heard all that the participants did.

After describing the purpose and the procedure of both studies, the experimenter explained that the materials for the opinion topics he was currently using "on the street" were reliable and valid. At this point the experimenter assigned the topic of air pollution to the participant and the topic of automobile safety to the confederate. The air pollution topic read: "Air pollution is really an important problem in New Haven and right now." Pretests indicated that the subject population was virtually unanimous in the opinion that air pollution was an important problem in New Haven and that something should be done about it.

### *Relevance of Belief to Behavior*

Immediately after the assignment of the topics, the experimenter said: "These topics are randomly assigned and I'd like you to take them, but if you *strongly* object, maybe we could work something out. Can I assume that you don't object to your topics?" He then looked squarely at the participant. All participants indicated passive agreement by either nodding or one-word agreement (e.g., "Okay" or "Sure"). The experimenter then turned his attention to the confederate, who responded to his query in one of two ways.

In the belief-relevant condition the confederate said: "You said this was reliable, right?" [Experimenter answered, "Yes."] "Okay, I wouldn't mind convincing people about something I really *believe in.* I guess that's the important thing."

In the belief-irrelevant condition the confederate said: "You said this was reliable, right?" ("Yes.") "Okay, it would be good to be in a study that really *shows something.* I guess that's the important thing."

Three confederates were used, all of whom had been subjects in a previous experiment conducted by the authors. Each confederate provided a replication of the design and their data were collapsed for analysis. Each confederate had his own random number table to determine the experimental condition. The experimenter was therefore unaware of the relevance condition until the confederate spoke up.

### *Dependent Measures*

*Opinion items.* Following the manipulation of relevance of beliefs by the confederate, the experimenter introduced the dependent measures:

> Before we break and you actually get down to business, there's one more thing we have to do—very quickly. I'm thinking about using Yale students as subjects, rather than as experimenters, in the next study I'm planning to do. The problem here is that I don't know how students feel in general about the potential issues I could use. What I need now is some specific information on what students' opinions are on the potential issues. I don't have time to get everybody's opinions on all the issues, so I'm just asking each person to tell me how he feels about three issues. . . . Here are three standard questionnaires on rat proliferation, air pollution, and auto safety—since I'm considering using these topics with Yale students.

For each of the three social issues there were items on how important the problem was, how much needed to be done, how great a threat to New Haven the problem was, and how much effort should be put into solving the problem. Each of the items was answered on an 11-point scale, where 11 represented the maximum opposition to air pollution. In addition, subjects were presented with a list of 10 social issues, including air pollution, and were asked to rank them with respect to how important it was that society solve the problems.

*Motivational items.* Several measures were obtained from participants under the guise of a departmental questionnaire. After the subjects filled out the attitude scales, the experimenter said:

> Okay, Mr. [Witness], let me get you started on your task—Oh, wait a minute—Why don't you two [referring to the confederate and the participant] fill out a departmental questionnaire now since you probably won't have time when you get back. The questionnaire is completely anonymous. I'm not allowed to look at them, so when you're done just put it in this envelope. . . .

The questionnaire was designed primarily to determine whether the experimental manipulation had motivational consequences for the participants. Questions referring to the experimental task were worded in the past tense in order to maintain the fiction that subjects were actually going to perform an experimental task. Subjects were asked to answer such questions as if they were written in the future tense. The questions assessed how much effort subjects expected to expend on the task, how well they thought they would do, whether or not they would volunteer for such a task again, and how much they thought they would enjoy it.

## RESULTS

It was the authors' anticipation that when the confederate implied that his behavior is dictated in part by his beliefs, the participant would accept the implication for himself. To the extent that the participant assumes that his own behavior is dictated by his beliefs, he should infer that he must believe strongly in the importance of combatting air pollution. Therefore, the authors expected subjects in the belief-relevant condition to express stronger anti-air-pollution sentiments than subjects in the belief-irrelevant condition.

The chief dependent variable was the scale consisting of summed responses to the four air-pollution items. Table 1 presents mean scores on the scale for the two participant conditions and the witness condition. It may be noted first that witnesses' attitudes toward air pollution did not differ as a function of the confederate's statements about the relevance of his belief to his behavior. Mean opposition to air pollution was 31.71 for witnesses who heard the belief-relevant communication and 29.33 for witnesses who heard the belief-irrelevant communication ($t < 1$). The two groups of witnesses have therefore been combined for purposes of analysis.

It may be seen in Table 1 that the belief-relevant participants were more opposed to air pollution than the belief-irrelevant participants.[3] The effect of hearing the confederate say that his belief was relevant to his behavior was to move participants 43% of the remaining distance toward maximum opposition to air pollution. The authors were apparently successful in creating a situation in which commitment to behavior had no effect by itself: the belief-irrelevant participants did not differ from the witnesses in their opposition to air pollution. In a situation where intended behavior has no consequences for belief, then, the present authors have been successful in imposing a link between behavior and belief. Where the link exists, opinion is altered by commitment. Where it does not, commitment has no effect on opinion.

Subjects were also asked to rank air pollution on its importance as a social problem. Here the pattern was also the predicted one, although considerably weaker. Belief-relevant participants ranked air pollution slightly higher as a social problem than belief-irrelevant participants (3.40 versus 4.12, $t = 1.13$,

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[3] All $p$ levels reported are based on two-tailed tests.

TABLE 1

OPPOSITION TO AIR POLLUTION

| Condition | Mean score | $t$ |
|---|---|---|
| Belief relevant | 37.00 (15) | |
| Belief irrelevant | 31.75 (16) | |
| Witness | 30.52 (27) | |
| Relevant vs. irrelevant | | 2.28* |
| Relevant vs. witness | | 2.66** |
| Irrelevant vs. witness | | <1 |

Note.—$n$'s are in parentheses. Range of possible scores is from 4 (minimal opposition) to 44 (maximal opposition).

* $p < .05$.

** $p < .02$.

*ns*) and than witnesses (3.40 versus 5.00, $t = 2.35$, $p < .025$). Belief-irrelevant subjects and witnesses did not differ significantly ($t = 1.20$, *ns*).

An analysis of attitudes towards auto safety is of interest in evaluating the data on attitudes towards air pollution. The confederate's statements had no noticeable effect on either participants' or witnesses' beliefs about the importance of promoting auto safety. For subjects who heard the confederate say he believed in promoting auto safety, the mean belief in the importance of auto safety was 32.14 (32.67 for participants, 31.57 for witnesses). For subjects who heard the irrelevant communication, the mean belief in the importance of auto safety was 29.79 (30.56 for participants, 28.85 for witnesses). The difference between the means for subjects exposed to the different manipulations is not significant ($t = 1.22$, $df = 56$, *ns*). The confederate's statements also failed to affect subjects' ranking of the auto safety question as a social issue. Thus, the confederate's communications about auto safety did not affect subjects' attitudes toward the topic, even though his opinion about the relevance of his beliefs to his behavior was apparently adopted by the participants.

Belief-relevant participants, then, accepted the link between behavior and belief which the confederate established and became more extreme in their belief. Did this enhanced belief in the importance of combatting air pollution lead to stronger motivation to proselytize against air pollution? Participants were asked, on the "departmental" questionnaire, how much effort they expected to expend on the task, how well they thought they would do, whether they would volunteer for such a task again, and how much they thought they would enjoy it. It was felt that the firs "effort" question was the item which mos directly assessed task motivation, but it wa felt that each of the other three items migh also tap motivation. The sum of the four item and the response to the effort item separatel are presented in Table 2. It may be seen tha subjects in the belief-relevant condition wer indeed more motivated to perform well on th experimental task than subjects in the belief irrelevant condition. All four item means ar in the same direction, although the effort item provided by far the best discrimination, bette in fact than the sum of all four items.

It might also be expected that task motiva tion would be strongly correlated with atti tude toward air pollution among belief-rele vant subjects, and less positively correlated for belief-irrelevant subjects. There are un doubtedly a variety of reasons for wanting to perform well on the task, but for belief-rele vant subjects the increased motivation was presumably a consequence of increased belie in the importance of combatting air pollution One therefore expects the correlation between motivation and attitudes to be more strongly positive for belief-relevant than for belief irrelevant subjects. It may be seen in Table 2 that this is the case. Again, the effort item shows by far the greatest differential effect.

The presence of a positive correlation be-

TABLE 2

MOTIVATION TO PERFORM EXPERIMENTAL TASK

| Item | Condition | | $p$ |
|---|---|---|---|
| | Belief relevant | Belief irrelevant | |
| Sum of four motivation items | 21.00 | 18.67[a] | <.06 |
| Effort item | 5.47 | 4.44 | <.02 |
| Correlation of four motivation items with opposition to air pollution | .33 | −.38[a] | <.07 |
| Correlation of effort item with opposition to air pollution | .69 | .14 | <.08 |

Note.—Responses to motivation items were on 7-point scales, with 7 indicating maximum motivation.

[a] One subject was left out of the analysis because he failed to answer one of the questions.

tween motivation and belief tells us nothing about the direction of causation, of course, and it should be noted that the present authors may be quite mistaken about the sequence of cognitive events which occurs for belief-relevant subjects. The authors have assumed that the sequence is the following: Confederate asserts that his behavior is dictated in part by his belief; subject assumes that his own behavior is dictated in part by his belief; subject infers from the fact that he is proselytizing on behalf of his beliefs that he holds extreme beliefs; subject's extreme beliefs motivate him to perform well on the task. However, there is nothing in the present data which would allow us to rule out a chain of events like the following: Confederate expresses strong motivation to proselytize for his beliefs; confederate's enthusiasm rubs off on the subject; subject infers from the fact that he is highly motivated to proselytize that he holds strong beliefs about the importance of combatting air pollution. With either sequence a belief inference takes place, but in the second sequence the inference is a consequence of motivational contagion, while in the first sequence the inference is a consequence of an altered perception of the meaning of the task.

## DISCUSSION

The present study provides support for the hypothesis that people on occasion infer their beliefs from their behavior. Or more precisely —since subjects did not actually carry out the behavior—the experiment provides evidence that people infer their beliefs from their behavioral intentions. The study also suggests that such an inference will not occur unless the individual feels that his behavior is dictated at least in part by his belief. Mere commitment to proselytizing behavior did not cause subjects to believe more strongly in the importance of combatting air pollution. It was necessary to introduce a cue of belief relevance for the belief inference to occur.

Viewed from another perspective, the present experiment indicates that a belief inference may often occur erroneously. The subjects' intentions to proselytize against air pollution had nothing whatever to do with their personal views on the topic. Yet when a model indicated that his intentions to perform a similar behavior were influenced by his convictions, subjects apparently assumed that they were similarly motivated by their beliefs. As a consequence of the error, subjects "overestimated" the extent of their opposition to air pollution. This sort of error may not be an infrequent occurrence. Whenever an individual finds himself performing consonant behavior in the company of truer believers than himself, he may become more entrenched in his belief and more highly motivated to perform the behavior.

It is profitable to place the present study in the context of the attribution theory advanced by Kelley (1967). Kelley proposes that a great many phenomena of attitude formation and motivation may be viewed as the outcome of the individual's attempt to discover the causes of the effects which he observes. In doing so, the individual often attributes erroneous causes to the observed effects. Thus, for example, Schachter and Singer (1962) injected subjects with a drug which produced autonomic arousal and then confronted them with a situation calculated to evoke an emotional response. Such subjects became more emotional than subjects who had been injected with the same drug but informed that it would produce arousal. In Kelley's terms, the uninformed subjects were the victims of an attribution error—they mistakenly attributed the autonomic arousal effect to the social situation in which they found themselves. It is equally possible to view the subjects in the present experiment as the victims of an attribution error. When led to think that a consonant behavior was relevant to belief, subjects "attributed" their behavior to their belief and erroneously became more extreme in their beliefs and more strongly motivated to perform the behavior.

The present authors have found support for Bem's hypothesis and feel that the results are readily interpreted in terms of Kelley's attribution theory, but such facts should not be taken to mean that the authors agree with Bem or Kelley about the role of belief inference in dissonance experiments. Kelley, although differing with Bem over particulars, has essentially embraced Bem's view that attitude change in dissonance experiments is a consequence of a passive inference rather than a motivated search for a justification of behavior. The present authors find it quite plausible that an individual might make such passive inferences within a narrow range on an attitude continuum, as in the present experiment where subjects were committed to consonant behavior and became slightly more extreme in their beliefs. But the behavior required of the subject in a dissonance experiment is counterattitudinal, which means that there is a very wide discrepancy between belief and behavior. The fact that he has behaved in a way which is inconsistent with his beliefs is almost certainly quite salient and vivid to him. In the face of such a patent contradiction it seems unlikely that he would passively infer that his attitude must not have been so very out of line with his behavior.

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# DISSONANCE AND RELATIVE VERSUS ABSOLUTE ATTRACTIVENESS OF DECISION ALTERNATIVES[1]

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When decisions are made, dissonance should increase (*a*) with the relative attractiveness of the unchosen alternative (postdecisional inconsistency), and (*b*) with the absolute value of the chosen alternative (decision importance). However, only relative attractiveness was consistent with expectations. Absolute attractiveness produced a U-shaped function instead of being monotonic. And, with alternatives close in relative attractiveness, decision confidence increased the spreading apart of the alternatives' attractiveness instead of decreasing it. More questions than answers were raised about postdecision reevaluation of decision alternatives.

When an attractive alternative is rejected, the dissonance which arises theoretically can be reduced by justifying the decision. To do this, one might make the chosen alternative seem even more favorable than it was originally. The rejected alternative can also be devalued. This type of dissonance reduction, "spreading apart the alternatives [Festinger, 1964, p. 164]," is measured by comparing a subject's rating of the two alternatives before and after the decision.

There are two key factors which theoretically regulate the amount of postdecision dissonance—"postdecision inconsistency" and "decision importance" (Festinger, 1957; see also Greenwald, 1968). Close relative attractiveness of the decision alternatives generates high inconsistency, sometimes termed conflict: the more relatively attractive the unchosen alternatives, the harder it is to justify the decision. The harder the justification, the greater the dissonance, and the more "spreading apart of alternatives" is predicted. The "importance" factor is illustrated by the absolute attractiveness of the alternatives: the more valued the alternatives, the greater the consequences of the decision and therefore the greater the need to justify the decision. Thus, greater spreading apart is predicted for higher absolute attractiveness, or for other importance manipulations.

Tests of "postdecision inconsistency" have generally been consistent with the theory (e.g., Brehm, 1956; Jecker, 1964; Penner, Fitch, & Weick, 1966), but tests of "decision importance" have not (e.g., Allen, 1964; Gerard, Blevans, & Malcolm, 1964).

Perhaps the theoretical effects for these two factors might emerge if relative and absolute attractiveness were tested within the same experiment. This would better control for each. Or perhaps consequences of the decision need to be explicitly manipulated in order for strong importance effects to occur. The present study combined these two approaches.

## EXPERIMENT I

Using "spreading apart" as the dependent variable, the three independent variables mentioned above—relative and absolute attractiveness of the decision alternatives and explicit consequences of the decision—were manipulated as described below.

### *Procedure*

*The premeasure.* In the experimental situation subjects were to choose between two popular singers. Therefore, 2 weeks prior to the experimental session, introductory psychology students rated 40 singers'

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acceptability on a 15-point scale (1 = only a bit acceptable, 15 = extremely acceptable). It was part of a larger opinionnaire. (Pretests had indicated that subjects' judging was aided by the term "acceptability.") A week later, students from several sections volunteered for a study in "auditory esthetic preferences." The next week these subjects came to the experiment in numbers of 8–18. They expected to enter a private booth, and there to evaluate the records of a popular singer for about an hour.

*Manipulation of explicit consequences of the decision.* In half the booklets, subjects were informed they were to choose the singer they were to evaluate (specific consequences decision, i.e., high-decision–importance). However, other subjects were asked simply to express their preference for one of the two singers (irrelevant consequences decision, i.e., low-decision–importance). In this latter condition, subjects were told that a record already had been selected for them by a random process. The singer's name was not given but awaited them in the booth. As Festinger (1964, p. 156) indicated, expressions of preference have few if any personal consequences, so should produce little or no dissonance.

*Manipulation of relative and absolute attractiveness.* The singers the subject chose between were derived from that subject's preratings. Relative attractiveness was manipulated by presenting a subject with two singers he had previously rated alike, 1 unit apart (e.g., 11-10 on the 15-point scale), or else two he had rated unalike, 5 units apart (e.g., 10-5). On the other hand, absolute attractiveness of the chosen singer was manipulated by making both singers attractive (e.g., 11-10), or both unattractive (e.g., 5-4). This resulted in four choice conditions: 5-4, 7-2, 10-5, and 11-10. Another condition (8-7 choice) will be described later.

This $2 \times 2 \times 2$ design contained 63 subjects.[3] The total $N$ was 94, and included 6 control subjects, 16 in the 8-7 choice condition, and 9 with unusable data. To aid the deceptions and reduce experimenter bias, printed manipulations were used, the booklets were distributed randomly, and subjects were run simultaneously.

*The dependent measure, "spreading apart."* Actually, subjects did not listen to records. Immediately after selecting a singer, they rated both singers with the same 15-point scale as in premeasure. Spreading apart was the increase in the chosen alternative's rating added to the decrease in the rejected alternative's rating. As a check on the ratings' reliability, the control subjects rerated several singers without making a choice.

After a postexperimental questionnaire and group interview, a briefing session was held in which the deceptions were discussed and subjects were pledged not to reveal anything about the experiment. It appeared that subjects did not have prior knowledge of the true nature of the study.

## *Results*

*Checks on the manipulations.* As noted, the levels of relative and absolute attractiveness were preselected to avoid overlap between the conditions. This distinctiveness was further suggested by different ($p < .005$) postdecisional changes in the ratings of both alternatives.[4]

Control subjects did not show systematic changes (all $p$ values were far from significant). In the specific consequences decision, 31 of the 32 subjects indicated they expected to listen to the singer they chose, while 27 of the 31 in the irrelevant consequences decision were uncertain which singer they would listen to ($p < .0001$). Thus the ratings seemed reliable and the inductions apparently effective.

*Effect of relative attractiveness on spreading apart.* Alike and unalike alternatives produced a highly significant effect. As Table 1 shows, mean spreading apart was 2.62 on the 15-point scale when the two alternatives were initially similar, that is, in the 5-4 and 11-10 choices. On the other hand, diminished spread of −1.30 occurred when the alternatives were initially different, that is, in the 7-2 and 10-5 choices ($F = 81.12$, $df = 1/55$, $p < .0001$). This high $p$ value was due largely to diminished spread among 68% of the low-conflict subjects, while 94% of the high-conflict subjects spread the alternatives apart ($\chi^2 = 40.15$, $df = 2$, $p < .0001$). Regression may have accounted for only part of this strong effect since $F = 43.70$ ($p < .0001$) after subtracting out the estimated regression effect. The latter was derived from the control group's changes.

This meant that after the decision, initial differences between conditions in the alternatives' relative ratings were all but eliminated. Looking at the final spread between the alternatives, one could not tell whether a subject had initially been in a high- or low-conflict decision (final spread = 3.09 and 3.22, respectively, $F = .08$, $df = 1/55$, $p = ns$).

Females diminished the spread more than did males when conflict was low (−2.34 and −0.65, respectively, $t = 2.67$, $df = 29$, $p <$

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[3] $n = 8$ per cell in all conditions of the $2 \times 2 \times 2$ design except for $n = 7$ in the 7-2 (irrelevant decision).

[4] Increase in the chosen alternative's rating was greater when absolute attractiveness was low than when it was high ($p < .005$).

TABLE 1

SPREADING APART IN TWO SINGERS' ATTRACTIVENESS RESULTING FROM RELATIVE AND ABSOLUTE ATTRACTIVENESS OF THE CHOICE

| Relative attractiveness of the 2 alternatives | Irrelevant decision consequences (simply expressing a preference) | | Specific decision consequences (choice affected hour's efforts) | | *M* | |
|---|---|---|---|---|---|---|
| | Low absolute attractiveness (5–4, or 7–2) | High absolute attractiveness (11–10, or 10–5) | Low absolute attractiveness (5–4, or 7–2) | High absolute attractiveness (11–10, or 10–5) | | |
| Alike in attractiveness (5–4, or 11–10) | 2.50 | 3.25 | 2.50 | 2.25 | 2.62 | ** |
| Unlike in attractiveness (7–2, or 10–5) | −1.57 | −0.75 | −1.37 | −1.50 | −1.30* | |
| *M* | 0.46 | 1.25 | 0.56 | 0.37 | 0.66 | |

Note.—$N = 63$. Per cell $n = 8$, except for $n = 7$ in the 7–2, irrelevant consequences decision. Each choice alternative was a popular singer rated on a 15-point scale piror to and after the decision.
* $p < .001$ with or without regression correction.
** $p < .0001$ with or without regression correction.

.02). This, the only effect due to sex, further suggests that regression may not have been the only factor diminishing the spread.

Expectations were not realized with the decision-consequences variable. Spreading apart was about the same regardless of whether the decision had consequences or was irrelevant (means of .47 and .86, respectively, in the wrong direction). Neither main nor interaction effects for this independent variable were significant (all $F$s $< 1$).

*Effect of absolute attractiveness on spreading apart.* The mean spread effects for choices of high and low absolute attractiveness were in the right direction ($M = .81$ and .51, respectively), but were far from significant ($F < 1$). This, too, was inconsistent with expectations.

Fortunately, an 8-7 choice condition had been included, providing an intermediate level of absolute attractiveness. This condition was analyzed as a 3 × 2 design with the other importance variable: choices of 5-4, 8-7, and 11–10, by specific versus irrelevant consequences. As Table 2 shows, increases in the choice's attractiveness produced a U-shaped function for spreading apart. Mean spreading apart in the 5-4, 8-7, and 11-10 choices was 2.50, 1.50, and 2.75, respectively (quadratic $F = 6.39$, $df = 1/45$, $p < .02$, trend analysis, Winer, 1962).[5] Put another way, mean spread-

TABLE 2

SPREADING APART IN TWO SINGERS' ATTRACTIVENESS AS A FUNCTION OF THE CHOICE'S ABSOLUTE ATTRACTIVENESS AND THE DECISION'S EXPLICIT CONSEQUENCES

| Explicit consequences of the decision | Absolute attractiveness of the choice (implicit importance of the decision) | | | | *M* |
|---|---|---|---|---|---|
| | Low attractiveness (5–4 choice) | Moderate attractiveness (8–7 choice) | High attractiveness (11–10 choice) | | |
| Irrelevant consequences (simply expressing a preference) | 2.50 | 1.62 | 3.25 | * | 2.46 |
| Specific consequences (choice affected hour's efforts) | 2.50 | 1.37 | 2.25 | | 2.04 |
| *M* | 2.50 | 1.50 | 2.75 | | 2.25 |
| | | ** | | | |

Note.—$N = 48$. Per cell $n = 8$. Each choice alternative was a popular singer rated on a 15-point scale prior to and after the decision.
* $p < .05$.
** $p < .02$.

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[5] The same U-shaped pattern also occurred in the

ing apart for the neutral and extreme choices taken together (2.62) was significantly greater than that for the moderately attractive choice (1.50; $t = 2.55$, $df = 46$, $p < .02$).

As before, the decision's explicit consequences did not provide a main effect, with means in the opposite direction ($M = 2.04$ and 2.46 for high and low consequences, respectively, $F < 1$, $df = 1/42$, $p = ns$). Within this main effect, however, one of the component simple effects was significantly opposed to the theory. As Table 2 shows, when the choice was attractive, explicit consequences significantly lessened the amount of spreading apart, that is, $M = 2.25$ compared to $M = 3.25$ when the consequences were irrelevant ($t = 2.26$, $df = 14$, $p < .05$).

Also contrary to expectations, the two types of importance did not add their effects. The high-high mean (2.25) was in fact less than the low-low mean (2.50; combined main effect $F < 1$, $df = 3/44$, $p = ns$). Nor was there an interaction between the two "importances" ($F = 1$, $df = 2/42$, $p = ns$).

In sum, the results showed the expected differential spreading apart with varying relative attractiveness, but unexpectedly revealed a U-shaped function for absolute attractiveness and a significantly opposed simple effect for explicit decision consequences.[6] These results will be discussed in a later section together with those of the second experiment below.

## EXPERIMENT II

A possible explanation for a U-shaped spreading apart function in the 5-4, 8-7, and 11-10 choices is that subjects may have more confidence in their ratings when these ratings are near anchor points of the scale. And, judging from Malewski (1962) and Gerard et al. (1964), perhaps the greater the confidence the greater the spreading apart.[7] To test this hypothesis, the following study was conducted.

### *Procedure*

A 2 × 2 × 2 experiment tested Conflict × Confidence × Explicit Decision Consequences. It was again hoped that the latter variable would produce the sought-after effect if circumstances were altered.

Eighty subjects, 59 experimental and 21 controls, were told their task was to standardize auditory rating scales. Each subject chose between two singers, expecting that he would listen to this singer's records. The two singers were those which the subject had rated alike or different in attractiveness at the beginning of the experimental session (high versus low conflict). Confidence in the decision was manipulated after their decision—but before their ratings—by such statements in their booklets as:

> Research has found that . . . there really can be no such thing as a wrong answer . . . . People have very good underlying reasons for the choices they make . . . especially . . . college students . . . [high-confidence condition] [or] Research has found that . . . people generally don't really know why they make the judgments they do . . . we are . . . extremely poor at being able to objectively justify why we choose one thing over another, why we like and why we dislike something . . . .

To manipulate the decision's explicit consequences, subjects were also told that they were likely (or unlikely) to participate in the study (high and low decision importance, respectively). The dependent variable, as in the previous study, was the degree to which the ratings of the two singers were spread apart after the decision.

### *Results*

*Reliability checks.* The degree to which subjects felt their decision was justified was significantly greater in the high- than in the low-confidence condition ($M = 7.47$ versus 4.79, respectively, on a 10-point scale, $F = 8.82$, $df = 1/45$, $p < .01$). (Data from six experimental and one control subject could not be used.) The degree to which subjects expected to participate in the study (decision importance) was significantly greater in the high- than in the low-participation condition ($M = 6.31$ versus 3.73, respectively, on a 10-point scale, $F = 11.57$, $df = 1/45$, $p < .001$). And, as before, the levels of low and high initial conflict (relative attractiveness) did not overlap, nor did the control group show systematic changes. Thus, the treatments seemed reliable and effective.

*Effect of confidence and conflict on spreading apart.* As in the first experiment, initial conflict had a powerful effect on spreading apart ($p < .001$). However, the other two

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final spread between the alternatives after the decision was over. Means were 3.06, 2.12, and 3.37, respectively, for low, moderate, and highly acceptable choices ($F = 6.64$, $df = 1/54$, $p < .02$). Initially, the spreads in these choices had been about equivalent ($M = .56$, .62, and .63, respectively, $F = .13$).

[6] The present results are equally puzzling from the viewpoint of conflict theory. Predictions for spreading apart are similar to those of dissonance theory.

[7] This idea emerged in discussions with Alvin Atkins and Irma Hilton.

TABLE 3

SPREADING APART IN TWO SINGERS' ATTRACTIVENESS AS A FUNCTION OF CONFIDENCE IN THE DECISION AND RELATIVE ATTRACTIVENESS OF THE CHOICE

| Confidence in the decision | Relative attractiveness of the choice | | | M |
|---|---|---|---|---|
| | Low conflict | Moderate conflict | High conflict | |
| Highconfidence in the decision | −1.03 (5) | 0.59 (18) | 1.63 (6) | 0.40 (29) |
| Low confidence in the decision | 0.08 (10) | 0.61 (8) | 0.64 (6) | 0.44 (24) |
| M | −0.47 (15) | 0.60 (26) | 1.13 (12) | 0.42 (53) |
| | | * | | |

Note.—Levels of each independent variable were determined by "natural groupings" of 12 or more scores of similar confidence or conflict. Scores based on a 15-point scale. *n*s in parentheses.
* $p < .001$.

variables in this study—manipulated confidence and importance—failed to reach significance levels. Subjects' responses to the check question on confidence were then used to split them into two levels of confidence. They were also split into *three* levels of initial conflict (relative attractiveness), resulting in a 2 × 3 (Confidence × Conflict) internal analysis.

In the results (see Table 3), confidence interacted with conflict ($F = 4.13$, $df = 2/47$, $p < .025$).[8] However, there was no main effect for confidence. And initial conflict was again significant.

Thus, spreading apart occurred when confidence was high if at the same time the decision was highly conflicted. And under presumed low conflict, when the initial difference between the ratings was large, self-confidence was associated with a *diminishing* of the spread. As noted below, these effects differ with dissonance theory's expectations.

## DISCUSSION

In dissonance theory, self-confidence should not increase spreading apart in high-conflict decisions. It should be an uncertain person who lacks justification for his decision and therefore seeks additional justification. A confident person need not justify his decision.

And the diminished spread found when conflict was low is also puzzling, especially the effect for high self-confidence. Regression does not appear to fully explain this result. Perhaps a kind of equalization of the alternatives occurs when decisions are made.

In any event, diminished spread appears to have occurred in other low-conflict studies whenever the initial spread has been sufficiently wide (e.g., Malewski, 1962; Gerard et al., 1964). If this phenomenon is indeed reliable, it is important because it means that inconsistency increased during or after the decision. This is incompatible with consistency theories, because inconsistency is thought to be discomforting and hence avoided, not sought.

But Brehm's (1962) finding that high-conflict choices were sought rather than avoided suggests that conflict may have a compelling aspect. (Perhaps this is related to seeking dissonant information—Freedman & Sears, 1965; Lowe & Steiner, 1968—and the nonshift of opinion which sometimes occurs in the face of extremely different viewpoints—Whittaker, 1963.) The frustrating results for decision consequences and absolute attractiveness illustrate the continuing difficulty of finding support for importance effects. Could it be that the importance variable has failed to support expectations because decision consequences increase the compelling as well as the disturbing aspects of conflict?

The data were in accord with one of dissonance theory's expectations, that regarding relative attractiveness (postdecision inconsistency, or conflict). Unfortunately, however, this result is not without alternative explanations. For example, a prominent one is Janis' (1959) conflict theory, which postulates predecision spreading apart (but see Footnote 6). Another explanation which seems to fit more comfortably with the present data is Lewin's (1952): "The act of decision . . . should be viewed as giving dominance to one of the alternatives [p. 465]." Presumably, this relates to any decision. It fits the present results because "dominance" could be greater the more confident a person is, and might be unaffected by the decision's importance.

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[8] Gerard et al.'s (1964) results also suggest an interaction, although an analysis apparently was not done.

## SUMMARY

The present investigation began as a test of dissonance theory's expectations regarding relative and absolute choice attractiveness (postdecision inconsistency and decision importance, respectively). In a $2 \times 2 \times 2$ experiment (Relative × Absolute Attractiveness × Explicit Decision Consequences), subjects were given a choice of popular singers. For some, the decision was supposed to provide them with records of that singer for the next hour's work; for others, it was simply expressing a preference (high and low consequences, respectively). The two singers were either similar or dissimilar in attractiveness (relative attractiveness). And absolute attractiveness was manipulated by arranging both alternatives to be low or high in attractiveness. The dependent variable was the degree to which subjects reevaluated the alternatives, measured by how far the chosen alternative was spread apart from the one which was rejected.

However, only relative attractiveness emerged as expected. As absolute attractiveness increased, spreading apart was U-shaped instead of monotonic. This latter result led to a second experiment testing the possibility that self-confidence may heighten spreading apart (confidence might have been greater for ratings near the scalar anchor points). Results suggested that confidence heightened spreading apart when conflict was high. But this is inconsistent with the theory because a confident person would not need to justify his decision. Diminished spread also occurred, although it should not have, because the discomfort of conflict theoretically should be avoided.

A number of these theory-inconsistent results occurred. They generated more questions than answers. It may be that additional approaches are needed to explain the reevaluation of decision alternatives.

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# DISSONANCE OR SOUR GRAPES?

## POST-"DECISION" CHANGES IN RATINGS AND CHOICE FREQUENCIES[1]

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Brehm's postdecision change paradigm was modified to require several decisions from each subject, to provide for measurement of postdecision changes in choice probabilities as well as in ratings, and to provide a control for the effects of changes in the availability of alternatives. The results support the fruitfulness of choice frequency as an alternative measure of post-"decision" changes, indicate that dissonance reduction is a probabilistic process, and cast doubt upon the importance of the act of decision (as opposed to a simple "sour grapes" effect) in generating changes in preference.

The most frequently replicated paradigm among studies of Festinger's (1957) theory of cognitive dissonance is that of Brehm (1956). In his postdecision change paradigm, each subject indicates (usually by ratings) his preference for each of a number of alternatives (electrical appliances, popular recordings, silk-screen prints, etc.) both before and after he has publicly stated which of two of the alternatives he wishes to receive. Despite the ubiquity of the finding of an increase in ratings of the chosen and a decrease in ratings of the rejected alternative (cf. Brehm, 1956; Brehm & Cohen, 1959; Festinger, 1964, Chapters 2, 3, and 5), a number of fundamental questions about the nature of the decision process remain unanswered.

Is a subject's choice within the single paired comparison requested of him deterministic or probabilistic? Dissonance theorists seem to favor the former interpretation (cf. especially Festinger, 1964, p. 2), despite the contraindication provided by the persistent phenomenon of "reversal subjects," that is, subjects who rate Alternative x higher than Alternative y on the initial set of ratings, but then choose y over x when the two are presented for a paired comparison. The deterministic view of the decision process, in addition to forcing invocation of measurement-scale unreliability or of sophomore irrationality in order to explain such reversals, implies a pessimistic view of the significance of the postdecision spread in ratings. If the only effect of a decision is to magnify a preference which is already sufficiently strong to insure 100% choice of the higher rated of the two alternatives, then the effect is a purely verbal one having no consequence for the subject's subsequent choice behavior with respect to the pair of alternatives. The present study, by employing paired comparisons as well as ratings in measuring each subject's preferences, sought to test whether postdecision changes in ratings might be paralleled by changes in choice probability. Demonstration of the fruitfulness of choice probability as a measure of changes in preference would have the additional advantage of making available to researchers in the area some of the more powerful models employed by mathematical learning theorists.

Harris (1967) presented three alternative models of postdecision dissonance phenomena based on Estes' stimulus-sampling theory (Atkinson & Estes, 1963; Estes, 1959). Each of the models generates slightly different sets of predictions about the relationship of the magnitude of postdecision changes to the predecision attractiveness of the chosen and rejected alternatives and about the distribution of such changes among chosen, rejected, and irrelevant alternatives. However, the directional predictions of the models do not differ greatly from those said to be derivable from verbal formulations of dissonance theory, and the magnitudes of the observed dissonance effects were too small to permit differential tests of the models' quantitative predictions. The models were useful, however, in pointing out gaps in verbal formulations of dissonance theory and in guiding the selection of appropriate statistical analyses.

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[1] This study is based primarily on a dissertation submitted to Stanford University in partial fulfillment of the requirements for a PhD degree. J. Merrill Carlsmith was principal adviser for this study. The author also wishes to thank William K. Estes, Richard C. Atkinson, and Jonathan L. Freedman for their helpful suggestions and advice. The manuscript was prepared during the author's tenure as Research Associate in the Behavioral Research Laboratory at the Palo Alto Veterans Administration Hospital, where it had the benefit of readings by Paul McReynolds and Charles Wilson.

[2] Now at the University of New Mexico. Requests for reprints should be sent to Richard J. Harris, Department of Psychology, University of New Mexico, Albuquerque, New Mexico 87106.

A second question of interest is the extent of individual differences in the applicability of dissonance predictions. That results averaged across subjects can be misrepresentative of the laws governing individual behavior is by now thoroughly established in the field of learning (cf. especially Bower, 1961; Bower & Trabasso, 1964). Atkinson, Bower, and Crothers (1965, Chapter 4) extend this caveat to the analysis of choice behavior. In the present study, large amounts of data, including the results of several decisions, were obtained from each subject so as to permit a test of dissonance predictions for individual subjects.

Finally, previous research on postdecision effects leaves uncontrolled the confounding effects of unavailability of the rejected alternative. Dissonance theory, especially in its more recent statements (Brehm & Cohen, 1962; Festinger, 1964), emphasizes the role of the subject's decision in committing him to receiving the chosen and giving up the rejected alternative as the crucial factor in producing postdecision spread. However, receiving anything might have a positive effect on attitudes toward the object received (the "ownership effect"), and learning that an alternative is no longer available might, as Festinger (1962) points out, lead by itself to devaluation of that alternative (the "sour grapes effect"). The ownership effect was tested by Brehm (1956), who found no greater increase in ratings of a gift than of other items the subject was asked to rate. The sour grapes effect was tested in the present study by making deletion of alternatives (long-playing recordings—LPs) contingent for half the subjects (the decision subjects) on their choices on each of several "critical" paired comparisons, while for the other subjects (the elimination subjects), the periodic deletions were unrelated to their judgments. The deletion of an LP from the set of alternatives available to an elimination subject bore no relation to any public decision, involved no *commitment* (as used by Brehm & Cohen, 1962) on the subject's part, and should have aroused little or no cognitive dissonance.

It might be argued that a decision subject's rejection of an LP should not arouse any cognitive dissonance either, since Jecker (1964) found that no net dissonance effect (increase in ratings of chosen plus decrease in ratings of rejected alternatives) occurred until all consequences of the decision (in particular, whether the subject would also receive the "rejected" alternative) were known. Jecker's results are, however, ambiguous since he failed to report separately the changes in the chosen alternative (which the subject knew he would receive) and in the rejected alternative. Allen (1964), in a replication and extension of the Jecker study, found that the chosen alternative did display the usual increase regardless of whether or not the fate of the rejected alternative had been determined. Thus postdecision change is "suspended" only with respect to those aspects of the decision whose consequences are unknown. Allen's (1964) results, then, suggest that no change in the subject's attitudes toward the "chosen" alternative should occur in the present study, since the subject may not actually receive the alternative and may indeed reject it on a subsequent trial. Devaluation of the rejected alternative can, however, be confidently predicted, since the subject knows that he will not receive that LP.

The present study, then, extended the standard postdecision change paradigm in order to determine whether the act of deciding between two alternatives has any influence on subsequent choice behavior, whether the typical findings (postdecision spread and greater change for choices between alternatives close in attractiveness) hold for individual subjects, and whether such effects are specific to subject's making a decision or are instead attributable to a sour grapes effect, that is, to changes in the availability of alternatives. A dividend of the methodological innovations required to examine the previous issues was an opportunity to test whether dissonance-reducing devaluation is an inevitable consequence of the rejection of an alternative or whether dissonance reduction is itself a probabilistic process. All previous studies have found that large percentages of subject-LP protocols in the high-dissonance conditions (e.g., 41% in Brehm's, 1956, maximum-dissonance condition) show postdecision changes in a dissonance-*increasing* direction. The finding might arise as a consequence of the variability of rating behavior, even though all decisions had led to dissonance-reducing changes in the "central tendencies" of the alternatives involved in the decision, or it might reflect a mixture of dissonance-reducing and dissonance-increasing changes in central tendency. A decision between the alternative explanations is possible only when (as in the present study) separate estimates of the variability of rating behavior and of the variability of the direction of postdecision changes are available.

## METHOD

### *Subjects*

Subjects were recruited through the Stanford University Employment Service and through notices posted on campus bulletin boards for "a psychology experiment on the factors underlying *preferences among LP recordings* and on methodological problems involved in measuring these preferences." Each subject was paid $1.50 per hour for his participation in the study.

Ten subjects were recruited and run, and the data for the 10 subjects were given a cursory analysis. Experimental procedures were altered, and 10 more subjects were recruited and run. The first 10 subjects constitute Experiment I; the second 10, Experiment II.

### *Ratings versus Paired Comparisons*

Each subject's principal task was to indicate how much he would like to own each of several LPs both in terms of a 6-unit rating scale and through a paired-comparison procedure. The basic stimuli were miniature reproductions of the album covers of various LPs offered by the Columbia Record Club; each reproduction was affiixed to a 3 × 5-inch yellow, ruled index card. The subject's judgments were actually elicited by the appearance in the read station of an IBM keypunch of a standard punch card containing the title of either one (for a rating) or two (for a paired comparison) of the LPs.[3] The instructions subjects were to follow in giving their ratings and in making their paired comparisons are reproduced in full in Harris (1967). The instructions included a general discussion of the "never the twain" character of previous research on rating-scale response and choice probability as measures of preference and the admonition to

> make your judgments as independent as possible. By "independent" I mean that each rating and each choice should reflect as accurately as possible your attitude towards the record or records involved in that judgment at that particular moment.

Each subject gave his ratings, to the nearest tenth of a unit, on a 6-unit scale anchored at 0 (dislike very much the thought of owning the LP), 3 ("neutral," neither like nor dislike the thought of owning the LP), and 6 (would like very much to own the LP).

The subject's judgments were broken down into *replications*. During each replication the subject gave a rating-scale response to each of 19 of the LPs and then made all 171 of the possible paired comparisons among the 19 LPs he had just read. The 19 recordings always included the 15 members of the constant set for that subject, plus one new LP the subject had not previously been asked to evaluate, plus three more LPs selected at random from those the subject had already judged at least once before.[4]

### *Dissonance Manipulation and No-Decision Control*

Each subject received one of the recordings at the end of the experiment. During each replication except the first two and the last two replications, one LP was deleted from the subject's choice set (the set of LPs from which subject would be permitted, at the end of the experiment, to select the LP he was to receive). Half of the subjects in each experiment (the elimination subjects) were told that which LPs were deleted from the choice set depended in no way on the judgments they made during the experiment. For the other half of the subjects (the decision subjects), however, one pair of the 171 in a given replication was selected at random as a "decision pair." The member of the decision pair that the subject did *not* choose was removed from consideration as the one he would receive at the end of the experiment. Each of the 15 constant-set items was included in each replication whether it had been deleted or not. Thus a number of ratings and paired comparisons on each deleted LP were available both before and after the deletion.

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[3] The author wishes to thank Gordon H. Bower for making the equipment available.

[4] Continual addition of new LPs was instituted at William K. Estes' suggestion as a means of preventing "early decisions," which Festinger and Walster (1964) found could have strong effects.

Unknown to the subjects, a yoking procedure (cf. Harris, 1967, for details) was employed to match the attractiveness of the LP deleted from an elimination subject's choice set on a given replication with the attractiveness of the LP rejected on that same replication by the decision subject with whom he was yoked.

Each subject had in front of him throughout the judgment sessions the album-cover reproduction of each of the LPs thus far introduced. As soon as an LP was eliminated from the subject's choice set, the subject was required to obtain that LP's serial number and record it, along with the album's title, on a list (headed either "Record of Rejections for Subject______" or "Record of Eliminations for Subject______") which he kept in front of him throughout the judgment sessions.

A decision subject's choice of x over y within a decision pair carried with it no commitment to receiving x. Such a choice did, however, commit the subject to giving up y. Thus only half of the usual dissonance manipulation was involved in each of the decisions—but such a procedure provided an economical way to obtain several decisions from each subject.

### *Differences between Experiment I and Experiment II*

Each of the 10 subjects in Experiment I completed 18 replications involving 14 deletions; each Experiment II subject completed 9 replications involving 5 deletions. In Experiment I, the LPs involved in critical pairs were selected from the full range of attractiveness (as measured by the subject's first replication ratings); in Experiment II the critical pairs were constructed from the more attractive LPs only. Each decision subject in Experiment I was informed immediately *after* his choice on each critical pair (by the appearance of an IBM card reading "The Last Pair Was a Decision Pair. You Have Rejected Record______") that that had been the decision pair; each Experiment II decision subject was informed immediately *before* its appearance of the imminence of a critical pair. Each of the procedural changes was designed to enhance the magnitude of the observed dissonance effects.

## RESULTS AND DISCUSSION

### *Relationship between Ratings and Paired Comparisons*

A previous study (Harris, 1965) established a fairly strong and consistent relationship between rating-scale responses and pairwise choice probabilities in individuals' judgments of personality-descriptive adjectives. The present study replicated that finding for the more concrete and more important task (for the subject) of choices among LP recordings.

The psychometric data are reported in Harris (1969). Two implications of the analysis bear emphasis, however. First, choice behavior is both probabilistic and deterministic. The subject responds deterministically with respect to the information available to him on any one trial; but, due to his limited information-processing capacity and the consequent fluctuation in that information from trial to trial, the subject responds probabilistically as observed across trials. As far as observable responses are concerned, then, *choices are probabilistic even as measured for individual subjects.* The overall mean probability of choosing the higher rated of two LPs differing by a full unit on a 6-unit scale was only .90. It thus seems at best superfluous to search for mental aberrations or slothful habits among those subjects who by chance are among the random 20% or so who choose the lower rated of two alternatives on a given paired-comparison trial and who thereby get themselves labelled as reversal subjects.

### *Effects of the Deletions on Subsequent Judgments*

In order to correct for any time trends in the data, a least-squares procedure was used to obtain two regression lines for each LP and each subject: one fitted to his ratings (or overall choice frequencies) before the trial on which that LP was deleted from the subject's choice set and one fitted to the postdeletion ratings (or choice frequencies). The difference between the predictions of the two regression lines for the trial of deletion was taken as the measure of the postdeletion dissonance effect for that subject-LP protocol. A $t$ test was run for each individual subject on the difference between zero and the observed postdeletion decrease displayed by his ratings (or choice frequencies). The overall significance of the rating (or choice frequency) decrease in each experimental condition was tested by the natural logarithm procedure for pooling $k$ independent tests of the same hypothesis (Winer, 1962, p. 43). The results of the tests are reported in Tables 1 through 4 for all 20 subjects of the present study.

The pattern of results is roughly the same whether measured in terms of ratings or in terms of overall choice frequencies. Statistically significant decreases are observed for only two individual subjects. Pooling across subjects, significant decreases are found for *both* the decision and the elimination groups in Experiment I and for *neither* group in Experiment II.

Three aspects of the data reported in Tables 1 through 4 are somewhat embarrassing to dissonance theory: the low magnitude of the observed "dissonance" effects, the generally smaller and statistically nonsignificant effects observed in Experiment II, and the nonsignificant differences between decision and elimination subjects.

*Magnitude of observed "dissonance" effects.* The magnitude of the observed postdecision decreases is somewhat unimpressive. Pooling over all decision subjects in both experiments, the overall percentage of LPs showing a postdeletion decrease was 55/93 = 59.1% when measured in terms of choice frequencies and 53/90 = 59.0% when measured in terms of ratings. Similarly, the overall mean decrease in ratings observed for decision subjects, averaged across all subject-LP protocols in the decision condition of both experiments, was .194 units on a 6-unit scale, or only 3.2% of the range of that scale.

TABLE 1

EFFECTS OF DELETION ON POSTDECISION RATINGS: INDIVIDUAL DATA

| Condition | Subject | $M$ | $s$ | $t$ |
|---|---|---|---|---|
| Experiment I | | | | |
| Decision | 1 | .77 | 1.21 | 2.40* |
| | 3 | .03 | .20 | .67 |
| | 4 | .04 | .38 | .42 |
| | 5 | .23 | .37 | 2.31* |
| | 9 | .06 | .62 | .38 |
| Elimination | 2 | .19 | .63 | 1.15 |
| | 8 | .05 | .17 | 1.17 |
| | 6 | .10 | .22 | 1.61 |
| | 7 | .10 | .35 | 1.12 |
| | 10 | .09 | .44 | .78 |
| Experiment II | | | | |
| Decision | 11 | .35 | .58 | 1.37 |
| | 12 | −.20 | 1.15 | −.18 |
| | 15 | .04 | .09 | 1.00 |
| | 17 | .45 | 1.05 | .95 |
| | 18 | −.13 | .48 | −.62 |
| Elimination | 14 | −.43 | .32 | −3.01* |
| | 13 | −.25 | .85 | −.67 |
| | 16 | .20 | .28 | 1.64 |
| | 20 | −.18 | .47 | −.90 |
| | 19 | 1.08 | 2.17 | 1.12 |

Note.—All tests reported in the present study are two-tailed.
$* p < .05$.

TABLE 2

EFFECTS OF DELETION ON POSTDECISION RATINGS: COMBINED TESTS

| Condition | Overall $M$ | $\chi^2$ | $df$ |
|---|---|---|---|
| Experiment I | | | |
| Decision | .228 | 27.5** | 10 |
| Elimination | .107 | 28.7** | 10 |
| Experiment II | | | |
| Decision | .100(.116) | 13.0(9.7) | 10(8) |
| Elimination | .087 | 7.6 | 10 |
| Experiments I & II | | | |
| Decision | .194(.203) | 40.5**(37.2*) | 20(18) |
| Elimination | .102 | 36.3* | 20 |

Note.—Parentheses indicate figures without Subject 15's data. (Cf. Harris, 1967, pp. 34–36, for a discussion of the peculiarities of Subject 15's ratings.)
$* p < .05$.
$** p < .01$.

The rather small decreases are, however, of the same order of magnitude as those reported in previous studies. Brehm (1956), for instance, found only 59% dissonance-reducing changes under conditions of maximum dissonance and a mean decrease for the rejected alternative of .30 units on an 8-unit scale, or 3.8% of the range of the scale.[5] Other studies have reported mean decreases for the rejected alternative of 2.1% (Allen, 1964),[6] 2.5% (Festinger & Walster, 1964),[7] and 4.0% (Walster, 1964)[8] of the range of the scale employed. The total dissonance effect (decrease in rejected plus increase in chosen alternative) observed in each of the studies is about twice the magniture of the effect for the rejected alternative alone.

*The probabilistic nature of dissonance reduction.* The magnitude of the dissonance effects observed in the present two experiments is not, then, inconsistent with the results of

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[5] The high- and low-dissonance groups have been pooled in obtaining the estimate.

[6] The low point of the 7-point scale was labelled "Don't care." The figure of 2.1% was arrived at by assuming that changes on this truncated 7-point scale were roughly equivalent to changes of the same magnitude on a nontruncated 14-point scale.

[7] Reversal and nonreversal subjects have been pooled in obtaining the estimate.

[8] The estimate is based on the 15-minute condition only.

TABLE 3

EFFECTS OF DELETION ON POSTDELETION CHOICE FREQUENCIES: INDIVIDUAL DATA

| Condition | Subject | $M$ | $s$ | $t$ |
|---|---|---|---|---|
| Experiment I | | | | |
| Decision | 1 | .73 | 1.65 | 1.60 |
| | 3 | −.06 | .60 | −.37 |
| | 4 | .08 | 2.24 | .13 |
| | 5 | .70 | 1.19 | 2.20* |
| | 9 | .43 | 1.09 | 1.48 |
| Elimination | 2 | .40 | .99 | 1.53 |
| | 8 | −.16 | .60 | −.99 |
| | 6 | .13 | 1.14 | .42 |
| | 7 | .27 | 1.00 | 1.00 |
| | 10 | −.00 | .60 | −.01 |
| Experiment II | | | | |
| Decision | 11 | .03 | 1.24 | .06 |
| | 12 | −.36 | 1.64 | −.48 |
| | 15 | −.41 | 1.74 | −.52 |
| | 17 | .32 | 2.79 | .26 |
| | 18 | .31 | .99 | .70 |
| Elimination | 14 | −.58 | 2.22 | −.58 |
| | 13 | −.32 | 1.00 | −.73 |
| | 16 | .38 | 2.12 | .40 |
| | 20 | −.19 | .90 | −.47 |
| | 19 | −.71 | 4.66 | −.34 |

* $p < .05$.

previous studies. However, it is surprising, given the observed mean effects and the large amount of data collected from each subject, that the postdeletion decrease is not statistically significant for more of the subjects. It is also surprising that such a high percentage of the LPs (over 40%) should show postdeletion *increases* in ratings. More surprising yet is that the much larger number of ratings per LP in Experiment I should not be reflected in greater reliability of the postdeletion decrease effect: if rejection always leads to a decrease in the "central tendency" of the subject's ratings of the rejected alternative, then the observed mean rating after rejection has a higher probability of being lower than the observed prerejection mean rating the greater the number of observations on which each mean is based. Instead, a slightly *lower* percentage (58%) of the Experiment I decision-condition subject-LP protocols show the anticipated decrease than in the decision conditions of Experiment II, where 60% decreases are observed. Such observations suggested the following test of the deterministic nature of dissonance reduction.

For each decision subject a pooled estimate of the within-LP variability of his ratings was obtained. The assumption of the present models that the number of elements conditioned to each LP decreases subsequent to its rejection is very nearly equivalent to assuming that the mean of the (normal) distribution from which the subject is drawing his ratings of the LP is always decreased subsequent to rejection of the LP. Assuming, as a first approximation, that the magnitude of the decrease is the same for each LP, the best estimate of the magnitude is, of course, the mean postdeletion decrease ($\bar{\Delta}$) actually observed for the subject. Given these assumptions, the probability that the mean rating ($\bar{R}_b$) before rejection is greater than the mean rating ($\bar{R}_a$) after rejection for an LP rejected on Trial $r$ is given by $Pr(\bar{R}_b > \bar{R}_a) = Q(z_r)$, where

$$Z_r = (0.0 - \bar{\Delta}) \Big/ \sqrt{s^2 \Big/ \left(\frac{1}{r} + \frac{1}{R-r}\right)},$$

$Q(x)$ is the area beyond $x$ under the unit normal curve, $s^2$ is the pooled estimate of the within-LP variability of the subject's ratings, $r$ is the replication in which the LP was rejected, and $R$ is the number of replications. This probability was calculated for each LP subject rejected. If the mean dissonance effect for a given subject was negative (as was the case for two of the Experiment II decision pair subjects), the probability was assumed to be equal to zero. The expected number of LPs showing a postdeletion de-

TABLE 4

EFFECTS OF DELETION ON POSTDELETION CHOICE FREQUENCIES: POOLED DATA

| Condition | Overall $M$ | $\chi^2$ | $df$ |
|---|---|---|---|
| Experiment I | | | |
| Decision | .376 | 26.2** | 10 |
| Elimination | .128 | 20.3* | 10 |
| Experiment II | | | |
| Decision | .022 | 7.6 | 10 |
| Elimination | −.284 | 6.3 | 10 |
| Experiment I & II | | | |
| Decision | .280 | 33.8[a] | 20 |
| Elimination | .019 | 26.6 | 20 |

[a] Two-tailed $\chi^2 = 34.2$, $p < .05$.
* $p < .05$.
** $p < .01$.

crease was then calculated by adding these probabilities together. For decision subjects in Experiment I such assumptions predict that 71% of the LPs will show the predicted postdeletion decrease. The difference between the previous prediction and the observed percentage of 58% is statistically significant at the .02 level ($\chi^2 = 5.74$, $df = 1$). The predicted percentage for decision subjects in Experiment II is 65%, which is not significantly different from the observed percentage of 60%. Pooled over all subject-LP protocols in both decision groups, the figures are 69% expected and 59% observed, $\chi^2 = 5.16$, $df = 1$, $p < .05$.

On the basis of these tests the assumption that dissonance-reducing devaluation is an inevitable consequence of rejection of an alternative must be rejected. The most likely alternative hypothesis, given the near-equality of percentage of decreases in the two decision conditions and the tantalizing occurrence of the same figure in Brehm (1956), is that rejection leads to devaluation with a probability of about .6 and to increased or unaltered attractiveness with a probability of about .4.

*Experiment I compared to Experiment II.* The second discomforting aspect of the data presented in Tables 1 through 4 is the generally smaller and statistically nonsignificant effects observed in Experiment II. The tendency for an LP to be rated lower and chosen less frequently after than before its deletion was statistically significant at at least the .05 level for both groups in Experiment I, but failed to reach even the .20 level for either group of Experiment II. A substantial portion of the failure to obtain statistically significant "dissonance" effects in the second experiment is attributable to the fewer data available for testing such effects in Experiment II. Nevertheless, obtaining lower mean effects in a study specifically designed to increase the magnitude of the observed dissonance effects is embarrassing for dissonance theory. The most likely explanation for the failure lies in the motivating effect of keeping the subject uncertain as to whether his choice had potentially important consequences. Decision subjects in Experiment I may have paid closer attention to all of the judgments, thus making dissonance more salient for such subjects.

*Dissonance versus sour grapes.* The most damaging aspect of the present data with respect to the theory of cognitive dissonance is their failure to demonstrate statistically significant differences between the decision and elimination groups. The emphasis on the role of commitment in arousing dissonance, as introduced in Brehm and Cohen (1962) and accepted by Festinger (1964, p. 156) strongly suggests that an elimination subject should *not* decrease his ratings or frequency of choice of an LP subsequent to its deletion, since the subject's knowledge of the various aspects of the eliminated LP is irrelevant to his knowledge that the experimenter has chosen to eliminate the LP. The difference between the decision and elimination subjects in Experiment I is in the right direction for both dependent measure and "approaches" statistical significance ($.05 < p < .10$) for the choice-frequency measure. The present data do not, however, provide convincing justification for labelling the observed attitude changes as post*decision* effects. None of the commonly cited studies of "postdecision dissonance effects" have included control data on alternatives made unavailable to the subject though not actively rejected by him. Thus, the changes in attractiveness observed in such studies may be attributable in large part to a "sour grapes" phenomenon, that is, to the discomfort created by the unavailability of an attractive alternative, rather than the commitment involved in actively rejecting that alternative.

Festinger (1962) acknowledged the possibility of a sour grapes effect, but only when the subject shares responsibility for the unavailability. Aronson and Carlsmith (1963) have, in fact, shown that when a child is prohibited from playing with a highly attractive toy either because the toy is removed from the room or because the child is threatened with severe punishment for playing with the toy, the child's rating of the attractiveness of the toy increases. If, however, the threat is a mild one, the child's ratings decrease. Aronson and Carlsmith's removal group seems closest in procedure to the elimination groups of the present study.

An incidental finding of another experiment (Walster & Festinger, 1964) suggests a possible explanation for the apparent discrepancy between Aronson and Carlsmith's (1963) study and the present one. Subjects in the Walster and Festinger study (second- and third-grade boys) rated a number of toys both before and after being told that one of the toys would not be available to them. Subjects required to select one of the available toys for themselves showed low recall of an initially attractive, but unavailable, alternative on a postdecision memory test and tended to remember more negative than positive aspects of the alternative when they did remember it. If the subject was simply required to state a preference among the available alternatives, or if the unavailable alternative was relatively unattractive, the effects were not as strong. The authors suggest that "these data support the contention that the subjects in the decision conditions had to push the 'ideal' alternative out of their minds in order to make a decision," and the differential recall of negative aspects suggests that they may, in addition, have devalued the unavailable alternative. A similar mechanism might have been involved in the elimination groups of the present study. Each elimination subject knew that he would, at the end of the experiment, have to make a decision among the remaining alternatives. Such an explanation, however, remains inconsistent with the theory of cognitive dissonance to the extent that the importance of commitment is considered a crucial aspect of the theory. It also contradicts the conclusion by Festinger (1964), a few chapters earlier in the same volume in which the Festinger and Walster results are reported, that no attitude change takes place in the predicision conflict period.

*Relation of Postdeletion Decrease to Predeletion Attractiveness*

The theory of cognitive dissonance and stimulus-sampling analogues of that theory have in common the prediction that the postdeletion decrease in ratings will be larger for initially attractive than for initially less attractive LPs. The precise mathematical form of the relationship was, in fact, to have been one of the principal tools for choosing amon the alternative models (Harris, 1967, Appendix A). A straightforward linear regressio analysis of the postdeletion decrease as function of each LP's predeletion mean ratin was run for each subject, and the differenc of the slope and of the intercept from zer were tested by the usual $t$ tests. The patter of results as reported in Table 5 is somewha ambiguous. The only single experimenta group in which the tendency for higher rate LPs to show greater postdeletion decreases i ratings reached statistical significance was th decision group of Experiment I. The tendenc was not, however, significantly more pro nounced for this group than for any othe

TABLE 5

REGRESSION OF DECREASE IN RATINGS ON MEAN PREDELETION RATING: COMBINED TESTS[a]

| Condition | $\chi^2_{B_0}$ | $\chi^2_{B_1}$ | $df$ |
|---|---|---|---|
| Experiment I | | | |
| Decision | 4.12 | 21.31* | 10 |
| Elimination | 7.20 | 17.74 | 10 |
| Experiment II | | | |
| Decision | 8.74 | 15.69 | 10 |
| Elimination | 22.36[b]* | 8.16 | 10 |
| Experiments I & II | | | |
| Decision | 12.86 | 37.00* | 20 |
| Elimination | 29.56 | 25.90 | 20 |

[a] Regression coefficients and $t$ tests for individual subjec are reported in Harris (1967, p. 42). The chi-square values a the end result of applying the natural logarithm procedu (Winer, 1962, p. 43) to the individual subjects $t$ tests to obtai a pooled estimate of the overall probability that the null hy pothesis of slope and intercept coefficients identically equal zero held for every subject.

[b] This statistically significant effect is due entirely to on subject's ratings of one LP. (Cf. Harris, 1967, p. 43, for discussion.)

* $p < .05$.

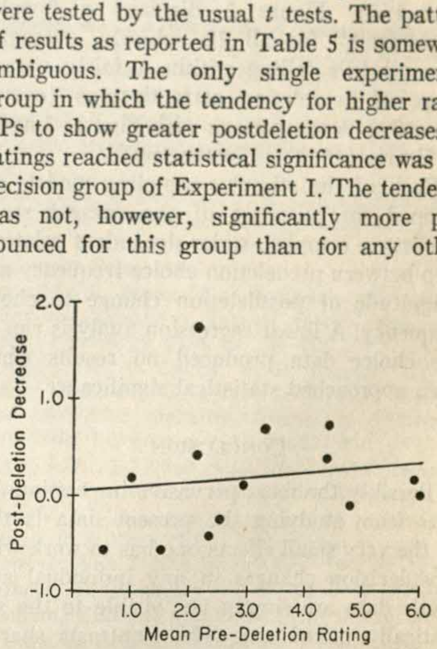

FIG. 1. Scatter diagram of postdeletion decrease i mean rating versus mean predeletion rating fo Subject 2.

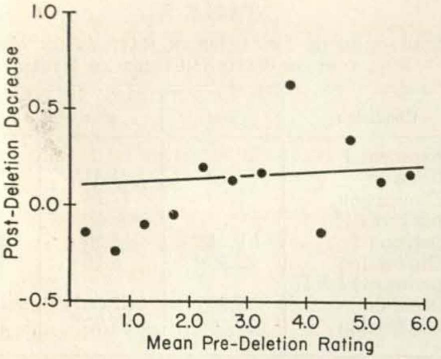

FIG. 2. Pooled scatter diagram for all 20 subjects of the present study.

and it does reach statistical significance for Experiment I as a whole and approaches significance for the decision-pair condition as a whole.

Figures 1 and 2 illustrate the magnitude of the relationships discussed in the present. Figure 1 displays the scatter diagram for Subject 2, who ranked fifth among the 10 subjects of Experiment I in terms of the Pearson product-moment correlation between predeletion ratings and postdeletion change ($r = .153$). Figure 2 displays a "pooled scattergram" for all 20 subjects, averaging over all LPs falling within a fairly narrow range of predeletion attractiveness (overall $r = .129$). A comparison with Figures 3 and 4 of Harris (1969) is instructive.

Each of the stimulus-sampling models developed for purposes of the present study predicts a complex, order-dependent relationship between predeletion choice frequency and magnitude of postdeletion change in choice frequency. A linear regression analysis run on the choice data produced no results which even approached statistical significance.

## CONCLUSION

Possibly the most pervasive impression one gets from studying the present data is that of the very small effects one has to work with. Postdecision changes in any individual subject's data are simply not visible to the statistically naked eye. This contrasts sharply with the relationship between ratings and choice probabilities, whose monotonicity is quite apparent in most subjects' data. Researchers working with post-"decision" phenomena had best look beyond significance levels and recognize that rather large quantities of data from each subject will be required to test any but the crudest hypotheses about dissonance-reduction processes.

The data and the statistical analyses discussed previously do, however, suggest an interesting (though highly tentative) picture of the behavior of an individual engaged in evaluating a number of alternatives. Each time he is asked to make a judgment, he thinks about (samples) a number of aspects of the various alternatives involved in the judgment. If he is to rate one of the objects, he performs a linear transformation on the proportion of all the aspects he has under consideration which favor the alternative. If the subject is to choose, or state a preference for, one of two objects, he chooses the alternative about which he knows, at that moment, more favorable things. His responses, then, whether ratings or paired comparisons, are deterministic with respect to the information available to him at any one time. Since the nature of that information fluctuates from time to time, his behavior across trials is probabilistic. If one of the alternatives is made unavailable, such a change in status may cause the subject to increase, decrease, or leave unaltered the number of cognitions (elements) he interprets as favorable to the eliminated alternative. There is a slight bias toward devaluation, the probability of a decrease in number of favorable aspects being about .6 and the overall effect (averaged across a number of eliminations) being a slight decrease. The magnitude of the decrease is greater for initially attractive alternatives. Whether this is due to a higher probability of devaluing such alternatives or to the greater number of favorable aspects available for reinterpretation cannot be determined from the present data. Both effects—the net tendency to decrease and the relationship of the magnitude of the decrease to initial attractiveness—occur whether or not the subject has anything to do with the elimination of the alternatives.

Such an interpretation of the results of the present study differs from existing verbal formulations of the theory of cognitive dis-

sonance in its less deterministic view of choice behavior and of dissonance reduction, in its implications for the problem of "reversal subjects," and in its derogation of the importance of commitment in bringing about attitude change. Support for some aspects of the interpretation is, however, quite weak. The present data clearly demonstrate that personal responsibility for loss of a desirable alternative is not a *necessary* condition for postloss devaluation, but that such commitment may be a *contributory* factor has certainly not been ruled out. The difference between the decision and elimination groups is in the direction predicted by dissonance theory both in Experiment I and in Experiment II, whether the decrease is measured in terms of ratings or choice frequencies, though none of the comparisons even approaches statistical significance. It could be argued that several features of the present study and especially of Experiment I—the use of alternatives covering the full range of attractiveness, the minimal importance of any single choice in the context of the 3,078 paired comparisons and 342 ratings required of each subject, the fact that the subject was informed only after his statement of preference that he had made a "decision"—insured that very minimal (if any) dissonance would be aroused. The strongest counterargument is that the basic results of previous studies were replicated in the decision group of Experiment I: postdeletion rating decreases of the same order of magnitude as previously observed and a significant tendency for higher rated alternatives to show a greater postdeletion decrease were both obtained. A fairer test of the contributory effects of decision-induced cognitive dissonance would, however, be possible if the importance of each decision could be increased and alternative modes of dissonance reduction (such as "I didn't know that choice would count") eliminated. Experiment II serves as an illustration of the difficulty of effecting such improvements without losing the ability to focus on results for individual subjects. In reducing the number of decisions, the amount of data collected per subject was drastically reduced. In restricting the choices to the more attractive alternatives, the power to detect a correlation of any given magnitude between
predeletion attractiveness and postdeletion d
valuation was reduced through restriction
the range of one of the variables. In warnin
the subject in advance of the imminence
a decision pair, his motivation to attend car
fully to any of the other choices was reduce
Apparently any increase in magnitude of dis
sonance provided by such changes in pro
cedure was insufficient to overcome the los
of statistical power they caused. Hopefully
future research designs will circumvent thes
problems and provide a more conclusive tes
of the relative importance of sour grapes an
cognitive dissonance effects in producin
devaluation of deleted alternatives.

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## Manuscripts Accepted for Publication in the
## *Journal of Personality and Social Psychology*

Reactions to Various Degrees of Deceit in a Mixed-Motive Relationship: Alan A. Benton,* Eric R. Gelber, Harold H. Kelley, and Barry A. Liebling: Department of Psychology, University of California, Los Angeles, P. O. Box 109, Los Angeles, California 90007.

Affective and Denotative Structures of Personality Ratings: Jorma Kuusinen*: Department of Psychology, University of Jyvaskyla, Jyvaskyla, Finland.

What is it That is Being Differentiated by the Semantic Differential? Murray S. Miron*: Department of Psychology, Syracuse University, Syracuse, New York.

On the Whys and Wherefores of E, P, and A: Charles E. Osgood*: Institute of Communications Research, University of Illinois, Urbana, Illinois 61801.

Perceived Parental Expressivity, Reaction to Stress, and Affiliation: Leon S. Robertson* and Louis E. Dotson: Harvard Medical School, Medical Care Research Unit, 83 Francis Street, Boston, Massachusetts 02115.

Aggressiveness and Psychophysiological Responsivity in Groups of Repressers and Sensitizers: Oscar A. Parsons,* Lawrence B. Fulgenzi, and Robert Edelberg: Division of Behavior Sciences, University of Oklahoma Medical School, 800 Northeast 13th Street, Oklahoma City, Oklahoma 73104.

Approach-Avoidance as a Function of Imagined Shock Threat and Self-Other Similarity: George Levinger*: Department of Psychology, University of Massachusetts, Amherst, Massachusetts 01003.

Effect of an Audience on Learning and Performance of a Complex Motor Skill: Rainer Martens*: 97 Children's Research Center, University of Illinois, Champaign, Illinois 61820.

Nonverbal Concomitants of Perceived and Intended Persuasiveness: Albert Mehrabian* and Martin Williams: Department of Psychology, University of California, Los Angeles, California.

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# EFFECT OF INITIAL SELLING PRICE ON SUBSEQUENT SALES[1]

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THOMAS K. LANDAUER,[4] AND SOLENG TOM, JR.
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Five field experiments investigated the effect of initial selling price on subsequent sales of common household products. Matched pairs of discount houses sold the same product at either a discounted price or the regular price for a short period of time. The prices were then made the same for all stores. The results were consistent with the prediction from dissonance theory that subsequent sales would be higher where the initial price was high.

The "introductory low price offer" is a common technique used by marketers. A new product is offered at a low price for a short period of time, and the price is subsequently raised to its normal level. Since the goal naturally is to maximize final sales of the product, the assumption behind this technique is that it will accomplish this goal. An economic model based entirely on supply and demand would of course predict that the eventual sales would not be affected by the initial price. The lower price would be expected to attract many marginal buyers and produce greater sales; but as soon as the price is raised, these buyers should drop out of the market. The hope of the marketer, however, is that some of these marginal buyers will learn to like the product enough so that they will continue to purchase it even at the higher price.

Unfortunately for the marketer, this may be a vain hope. There are various psychological reasons why we might expect the introductory low price to have an opposite effect from that which the marketers intend, such that the introductory low price would reduce rather than increase eventual sales. Since this technique is so widespread, it provides an unusual opportunity to investigate the applicability of social psychology in a natural setting, and to compare the marketer's predictions with that of social psychology.

The most interesting analysis of this situation is based on the theory of cognitive dissonance (Festinger, 1957). One of the clearest deductions from the theory is that the more effort in any form a person exerts to attain a goal, the more dissonance is aroused if the goal is less valuable than expected. The individual reduces this dissonance by increasing his liking for the goal, and therefore the greater the effort, the more he should like the goal. This prediction has received some substantiation in laboratory experimentation (e.g., Aronson & Mills, 1959; Gerard & Mathewson, 1966). Its applicability to the marketing situation is straightforward: the theory predicts that the higher the price a person initially pays for a product, the more he will come to like it. Presumably this greater liking will produce "brand loyalty" in the form of repeat purchases. Thus, when the initial price is high, a higher *proportion* of buyers should continue to purchase the product than when the initial price is low. Accordingly, although the introductory price will initially attract more customers, we may expect the sales curves for the two conditions to cross at some later point, and the higher brand loyalty induced by the dissonance involved in paying a high price to manifest itself in higher final sales in that condition.

Five experiments were performed to demonstrate that introducing a new brand of a product at a low price for a short time and then raising it to the normal selling price leads to lower sales in the long run than introducing the product at its normal selling price. The general design of all the experiments was to introduce the new brand at a low price in one set of stores and, after the price is raised to the normal selling price, compare sales with matched stores where the product was introduced at the normal selling price and held there throughout the course of the experiment.

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[1] This study was supported in part by National Science Foundation grants to Carlsmith and to Freedman. The authors are grateful to management and personnel of the discount chain for their cooperation in this research.

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All of the experiments that are to be reported here were done in a chain of discount houses. All sales figures have been multiplied by a constant in order to maintain confidentiality.

This chain of discount houses differs from most others in a number of important ways. They do not advertise much, and what advertising they do does not include prices on specific items. Price changes occur very seldom in these stores and are usually not advertised. In most cases, prices are lowered because an item is overstocked, and unless the customer remembers the regular selling price, he has no way of knowing that the price is lower than usual. Management in most of these stores is under direct control of the central office. When the manager receives orders from the central office, he has little or no power to change them.

The chain sells a large number of "house brands" at prices lower than the equivalent name brands. These house brands have the same registered trademark, and constitute a brand which customers can easily identify with the store. Generally, the quality of the house brand item is as high as the equivalent name brand, the differences usually being in characteristics which do not directly affect the usefulness of the item (e.g., mouthwash bottles are not as attractive as those of the name brand; the average grain size of powdered detergent is larger than that of the name brand which is chemically equivalent).

The products used in the studies reported here were house brands. All were being introduced into the stores at the time when the study was being run. The particular products used and the price differential were both determined by management.

## EXPERIMENT I

### *Method*

Twelve pairs of discount houses, matched on gross sales, were randomly assigned to one of two experi-

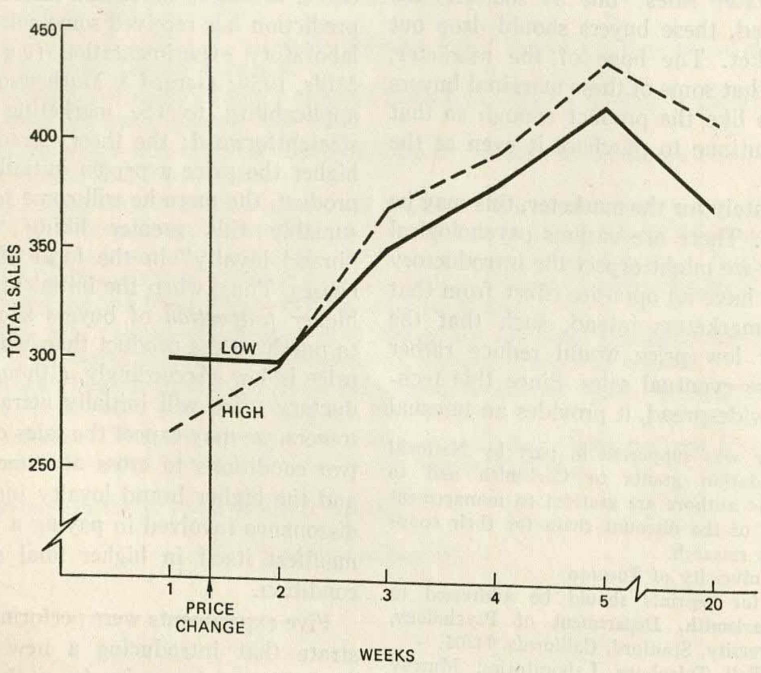

FIG. 1. Mouthwash sales.

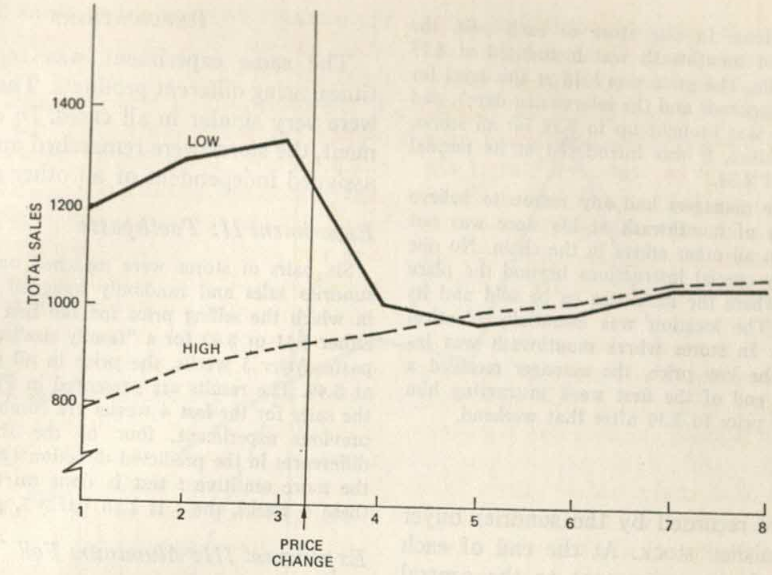

FIG. 2. Toothpaste sales.

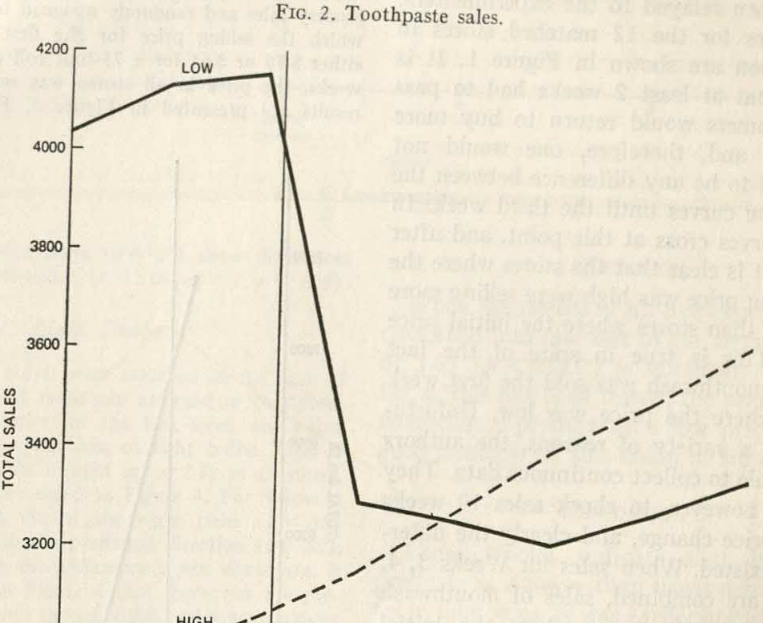

FIG. 3. Aluminum foil sales.

mental conditions. In one store of each pair, the house brand of mouthwash was introduced at \$.25 per quart bottle. The price was held at this level for 9 days (two weekends and the intervening days), and then the price was brought up to \$.39 for all stores. In the other store, it was introduced at its normal selling price of \$.39.

None of the managers had any reason to believe that the price of mouthwash at his store was not the same as in all other stores in the chain. No one was given any special instructions beyond the place in the store where the item was to be sold and its selling price. The location was essentially identical for all stores. In stores where mouthwash was introduced at the low price, the manager received a memo at the end of the first week instructing him to change the price to \$.39 after that weekend.

### *Results*

Sales were recorded by the sundries buyer as he replenished stock. At the end of each week these figures were sent to the central office and then relayed to the experimenters. Average sales for the 12 matched stores in each condition are shown in Figure 1. It is estimated that at least 2 weeks had to pass before customers would return to buy more mouthwash, and, therefore, one would not expect there to be any difference between the height of the curves until the third week. In fact, the curves cross at this point, and after this point, it is clear that the stores where the initial selling price was high were selling more mouthwash than stores where the initial price was low. This is true in spite of the fact that more mouthwash was sold the first week in stores where the price was low. Unfortunately, for a variety of reasons, the authors were not able to collect continuous data. They were able, however, to check sales 19 weeks after the price change, and clearly the difference still existed. When sales for Weeks 3, 4, 5, and 20 are combined, sales of mouthwash were higher in the store where the initial selling price was high in 10 of the 12 pairs of stores ($p = .02$).

Sales in the two sets of stores during Weeks 3, 4, 5, and 20 (pooled) were also compared by use of a $t$ test, resulting in a $t$ of 2.11 ($df = 11$, $p < .10$). Thus, stores where the initial selling price was low sold less mouthwash than did stores where the initial selling price was the same as the final selling price.

## REPLICATIONS

The same experiment was repeated four times, using different products. The procedures were very similar in all cases. In each experiment, the stores were rematched and randomly assigned independent of all other replications.

### *Experiment II: Toothpaste*

Six pairs of stores were matched on the basis of sundries sales and randomly assigned to conditions in which the selling price for the first 3 weeks was either \$.41 or \$.49 for a "family size" tube of toothpaste. After 3 weeks, the price in all stores was set at \$.49. The results are presented in Figure 2. When the sales for the last 4 weeks are combined as in the previous experiment, four of the six pairs show differences in the predicted direction ($p = .34$). When the more sensitive $t$ test is done on the data from these 4 weeks, the $t$ is 2.26 ($df = 5$, $p < .10$).

### *Experiment III: Aluminum Foil*

Seven pairs of stores were matched on the basis of grocery sales and randomly assigned to conditions in which the selling price for the first 3 weeks was either \$.59 or \$.64 for a 75-foot roll of foil. After 3 weeks, the price in all stores was set at \$.64. The results are presented in Figure 3. For Weeks 5–8

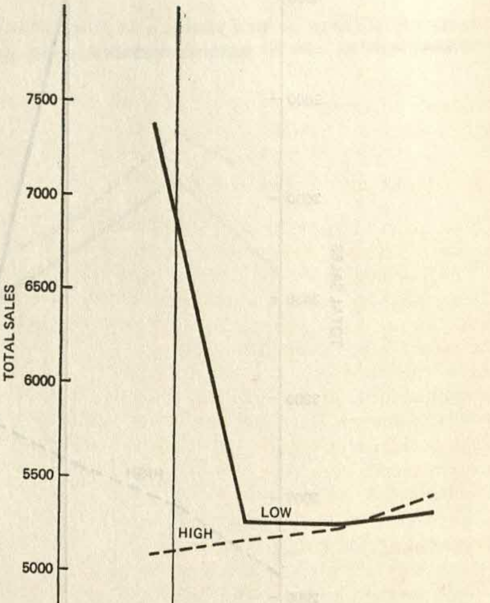

FIG. 4. Light bulb sales.

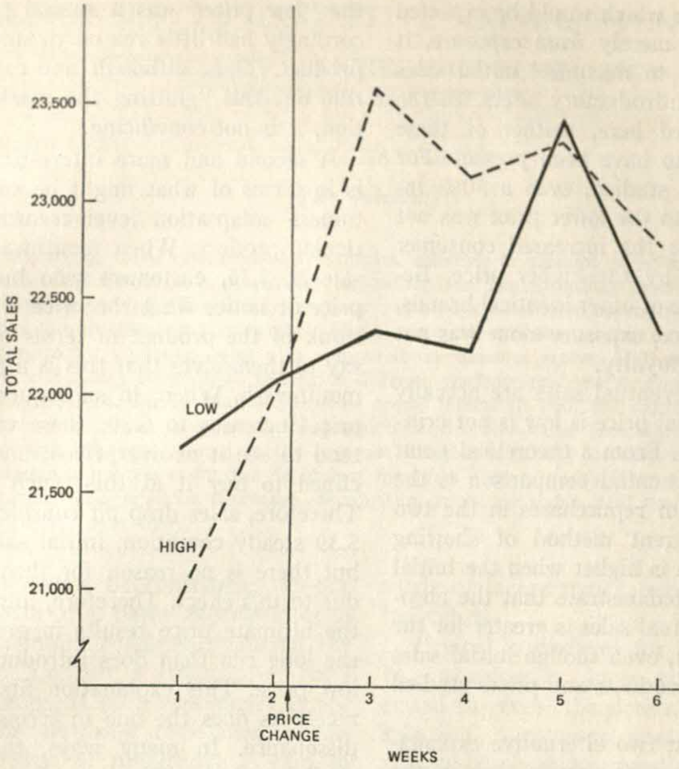

FIG. 5. Cookie sales.

combined, all seven pairs ($p = .01$) show differences in the predicted direction ($t = 5.09$, $df = 6$, $p < .005$).

### *Experiment IV: Light Bulbs*

Eight pairs of stores were matched on the basis of hardware sales and randomly assigned to conditions in which selling price for the first week was either \$.26 or \$.32 for a package of light bulbs. After 1 week, the price was brought up to \$.32 in all stores. The results are presented in Figure 4. For Weeks 3 and 4 combined, six of the eight pairs ($p = .15$) show differences in the predicted direction ($t = .837$, $df = 7$). Although this difference is not significant, it might be noted in Figure 4 that there was the predicted reversal, even though initial sales were almost 50% higher at the low price.

### *Experiment V: Cookies*

Eight pairs of stores were matched on the basis of grocery sales and randomly assigned to conditions in which the selling price for the first 2 weeks was \$.24 or \$.29 for a large bag of cookies. After 2 weeks, the price was at \$.29 for all stores. The results are presented in Figure 5. For Weeks 4–6 combined, six of the eight pairs show differences in the predicted direction ($t = .625$, $df = 7$).

## RESULTS

When the results of all five experiments are combined into one test of the hypothesis, a $z$ of 3.63 ($p < .0002$) is obtained. Clearly, so far as this has been tested, the practice of introducing a product at a low price is not a good strategy for this chain of stores to use.

## DISCUSSION

These studies indicate that introducing products at a lower than usual price is harmful to final sales. It was earlier argued that one possible reason for this is the lower proportion of buyers who return to a product when the initial price is lower than the normal price. Whether or not this causes eventual sales actually to be lower when the initial price is low is not critical to the argument. If, for example, there is an extremely large difference in initial sales, even a lower proportion of returning buyers may produce an advantage for the initial low price. Similarly, if the product has some special feature which would be expected to produce loyalty merely from exposure, it would be beneficial to maximize initial sales by the use of low introductory offers. In the experiments reported here, neither of these possibilities seems to have been present. For the range of prices studied, even a 50% increase in sales due to the lower price was not enough to overcome the increased consumer loyalty engendered by the higher price. Because of the presence of other identical brands, differing only in price, exposure alone was not enough to produce loyalty.

Whether or not eventual sales are actually lower when the initial price is low is not critical to the argument. From a theoretical point of view, the only essential comparison is the relative proportion of repurchases in the two conditions. A stringent method of showing that this proportion is higher when the initial price is high is to demonstrate that the absolute volume of eventual sales is greater for the high-price condition, even though initial sales are lower. For the products and prices studied here, this was true.

There are at least two alternative explanations of this result. The first is that in the low-initial-price stores the market is glutted after the first few weeks, and it takes a long time for there to be any need to repurchase the product. This might be a partial explanation of the difference between the conditions, but seems implausible as a total explanation. For all the products except light bulbs the length of time that the sales curves were followed exceeded by a goodly margin the marketer's estimate of the normal time until repurchase. Indeed, with mouthwash, for which the repurchase period is about 2 weeks, the difference between conditions is still present 19 weeks after the price switch. Customers might have stocked up by buying more than their usual supply of a product, but pricing practices of this chain of stores makes this unlikely. These stores have rarely used low introductory price offers and they were not advertised as such for the products studied. Buyers therefore had no reason to believe that the "low price" was a special price and accordingly had little reason to stock up on the product. Thus, although one cannot entirely rule out this "glutting the market" explanation, it is not convincing.

A second and more interesting alternative is in terms of what might be called the customers' adaptation level regarding the particular product. When mouthwash is put on sale at $.25, customers who buy it at that price or notice what the price is may tend to think of the product in terms of $.25. They say to themselves that this is a $.25 bottle of mouthwash. When, in subsequent weeks, the price increases to $.39, these customers will tend to see it as overpriced, and are not inclined to buy it at this much higher price. Therefore, sales drop off considerably. In the $.39 steady condition, initial sales are lower, but there is no reason for them to drop off due to this effect. Therefore, introducing it at the ultimate price results in greater sales in the long run than does introducing it at the low price. This explanation fits the data as nicely as does the one in terms of cognitive dissonance. In many ways, they are quite similar and are difficult to distinguish experimentally.

It should be noted that the adaptation level and dissonance explanations are by no means mutually exclusive. It is entirely possible that both mechanisms are operating to some extent. In any case, the basic result stands—the introduction of a product at a low price tended to decrease subsequent sales, and this effect lasted for at least 20 weeks.

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# RESPONSES TO COGNITIVE INCONSISTENCIES:

## PERSONALITY, DISCREPANCY LEVEL, AND RESPONSE STABILITY [1]

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Subjects in three experimental treatments, differing in amount of discrepancy between subjects' own personality self-ratings and bogus ratings presented to them, responded differentially on four response measures: conformity, rejection, devaluation, and underrecall. High, moderate, and low groups on several personality attributes were also compared on response scores. Differences in Love-Hostility scores were related to different response preferences, and classification according to Marlowe-Crowne scores interacted with the discrepancy-level treatments. Dogmatism, Repression-Sensitization, and Dominance-Submission were not related to the response variables. Stability of response was examined by reassessing the dependent measures after a week's interval. Conformity and rejection decreased, devaluation remained stable, and underrecall increased over time.

The various forms of consistency theory have in common the basic postulate of a tendency to maintain consistency among the elements of one's cognitive structure or one's social relationships. If such consistency is not maintained, the resulting imbalance, asymmetry, incongruity, or dissonance is said to create within the person a tension or motive to eliminate the inconsistency and to restore balance.

A number of theorists have specified possible methods of reducing inconsistency (Abelson, 1959; Festinger, 1957; Newcomb, 1953; Steiner, 1966), but have failed to indicate the conditions under which each of the available alternative resolutions is most likely to occur. Several writers (Glass, 1968b; McGuire, 1966; Steiner, 1966) have argued that both situational and personality factors need to be considered as determinants of the mode of inconsistency reduction used by the individual.

Typical procedures for studying responses to inconsistency have not been optimally suited to reveal the determinants of response selection. A common strategy in consistency studies has been to investigate one particular mode of inconsistency reduction while experimental manipulations are employed to make alternative responses impossible or unlikely. The use of such "blocking" tactics has been criticized as leaving the results vulnerable to alternative explanations and as leading to such complex experimental conditions that results cannot easily be generalized (McGuire, 1966; Rosenberg, 1965). Furthermore, if one is interested in personality effects on the resolution of inconsistency, the restraints imposed by such manipulations can force a subject to use an atypical (for him) response. Hence, the results are likely to be distorted and, at the very least, generalization is limited. For such reasons, Steiner (1966) has emphasized the importance of creating experimental situations which permit the measurement of several modes of response.

Although an increasing number of researchers have assessed several response alternatives in their experiments, the most appropriate methods of data analysis have rarely been used in such studies (McGuire, 1966). When several responses are regarded as alternative means of coping with inconsistency, statistical methods should be used which consider the several response measures simultaneously in the analysis. This suggests that techniques such as multivariate analysis of variance and discriminant analysis would be most relevant.

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[1] This study is based on a dissertation submitted to the Graduate College of the University of Illinois in partial fulfillment of the requirements for the PhD degree. The author expresses his gratitude to Ivan D. Steiner for his guidance and consultation throughout the research, to Ledyard R. Tucker for his advice concerning the statistical analyses, and to Mark Appelbaum for assistance in the computer analysis of the data. The study was supported by United States Public Health Service Grant M-4460, Ivan D. Steiner, principal investigator.

[2] Requests for reprints should be sent to David L. Hamilton, Department of Psychology, Yale University, 333 Cedar St., New Haven, Connecticut 06510.

In the investigation of personality differences in the resolution of inconsistency, experimenters have frequently restricted their comparisons to high and low groups on selected personality variables. Such a procedure ignores the possibility that complex relationships may exist between personality and response variables.

Another issue concerns the stability of one's response in resolving inconsistency. Several studies have reported a decrease in conformity over time, and personality correlates of different types of conforming behavior have been found (Greenbaum, 1966; Harvey, 1965; Klein, 1967; Steiner & Vannoy, 1966). Little research on the stability of responses other than conformity has been reported (Steiner, Anderson, & Hays, 1967).

The investigation reported in the present paper attempted to overcome some of the problems in the study of personality attributes related to modes of resolving cognitive inconsistencies. High, moderate, and low groups on several personality variables were compared in terms of four responses to three amounts of experimentally created inconsistency. Since the four responses were viewed as alternative methods of reducing dissonance, multivariate techniques which permitted simultaneous analysis of all dependent variables were employed. Finally, the stability of the responses over a week's time was examined.

The personality dimensions included in the study were defensiveness (expressed in socially desirable responding), Dominance-Submission, Love-Hostility, Repression-Sensitization, and Dogmatism. The theoretical basis for selecting each of the variables is presented elsewhere (Hamilton, 1968).

## METHOD

### *Subjects*

Subjects in the experiment were 243 male undergraduate students. Of these, 156 were drawn from an introductory psychology course and received course credit for their participation. The other 87 subjects were paid for participating in the experiment. Data obtained from 12 subjects were discarded because postexperimental inquiries indicated suspicion or disbelief of the experimental manipulations employed. Two subjects were eliminated because data needed for measuring the dependent variables wre incomplete. Thus, the number of subjects considered in the data analyses was 229. Of these, 161 participated in one of three experimental conditions, while the remaining 68 served as a control group.

Three experimental sessions were conducted. Approximately 1 week intervened between the successive sessions.

### *First Session*

When the subject arrived for the first session, he was given a packet of test materials. In the test packet were the following personality measures: (*a*) the Repression-Sensitization (*R-S*) scale (Byrne, Barry, & Nelson, 1963), (*b*) the Marlowe-Crowne Social Desirability scale (M-C-SDS—Crowne & Marlowe, 1964), (c) the Dogmatism Scale (Rokeach, 1960), and (*d*) *t*he Interpersonal Check List (Leary, 1957).

Included among the inventories was a questionnaire containing 12 bipolar rating scales concerned with general personality and behavioral characteristics on which the subject was to rate himself. Each characteristic was presented on a 15-point scale with only the most favorable and least favorable points labeled at opposite ends of the continuum (e.g., extremely friendly–extremely unfriendly). The subject's ratings on the scales were used both in the experimental manipulation given to the subject and as a premeasure for one of the dependent variables (explained subsequently).

When the subjects had completed the test materials, they were told that they were participating in a project designed to evaluate the ability of graduate students in clinical psychology to score and interpret personality tests. They were informed that the graduate students would score the questionnaires they had just completed and then rate the subjects on a number of characteristics related to personality and behavior. The ratings were to be returned the following week.

Subjects then completed 10 rating scales. Five items asked for the subject's evaluations of the competence of the "typical psychology graduate student to interpret personality tests," and five were concerned with the validity and usefulness of the personality tests.

### *Second Session*

At the beginning of the second experimental session, held 1 week after the first session, the experimenter passed out a set of fictitious ratings to each of the experimental subjects. The ratings were made on the previously described set of 12 rating scales on which the subjects had rated themselves in the first session. The bogus ratings had been constructed by the experimenter in a systematic manner designed to create three experimental treatments. The treatment variable was the extent of discrepancy between the ratings prepared by the experimenter and the intial self-ratings made by the subject in the first session. The amount of discrepancy was a constant for all subjects in a given treatment condition. For example, in the slight discrepancy condition, the subject's self-ratings on six items were lowered one step on the 15-point scale, and on three items were lowered two steps. In the moderate discrepancy condition, self-ratings were lowered four steps for six items and five steps for three items. In the extreme discrepancy condition, self-ratings were lowered seven steps for six items and eight steps for three items. (The six items that were lowered by the lesser of the two amounts were the same items for all treatment conditions, as were the three items that were lowered by the greater amount.) In *all* treatments, self-ratings on the remaining three items were raised one step. This was done to preserve the face validity of the manipulated ratings. (These three items were disregarded in determining any of the dependent variable scores derived from the questionnaire form.) The experimental subjects were randomly assigned to the three treatment groups.

After the subjects had had time to examine the bogus ratings, the rating forms were collected, and additional questionnaires were distributed to the subjects. The first form consisted of 10 rating scales, similar to those completed at the end of the first session, concerning the competence of the graduate student and the validity of the tests. The only change in the items was on those scales evaluating the graduate student; instead of asking about the "typical psychology graduate student," the items referred to the "graduate student who rated you." The second form which the subjects completed at this time was the set of 12 personality ratings scales they had filled out in the previous week. This constituted another set of self-ratings made by the subject.

At this point, some of the common uses of psychological tests were pointed out to the subjects, and they were asked to write a few paragraphs indicating their views regarding the applicability of such tests in various situations. Approximately 10 minutes were allowed for the subjects to complete the task, which was included for two reasons: (*a*) It had the effect of lending further credence to the total deception, and (*b*) more importantly, it delayed the administration of the final set of rating scales. The final form consisted of the same 12 rating scales used in earlier measures. This time, however, the subject was asked to mark on them the ratings they had "received from the graduate student." That is, they were to indicate the evaluations which had been presented to them at the beginning of the session. When the subjects had completed the scales, the second session was ended.

### *Third Session*

Two hundred and five subjects returned for the third session, which took place 1 week after the second experimental session. In the third session, subjects completed the same measures they had filled out the previous week. That is, subjects again made evaluations of the graduate student and the tests, rated themselves on the 12 personal characteristics, and recalled how the graduate student had evaluated them on the rating scales. A final page asked questions designed to detect those subjects who either had not believed the deceptive material and experimental manipulations (i.e., the fictitious ratings) or had become aware of the reactions actually being studied.

When the subjects had completed the materials, the experimenter explained in detail the actual purpose of the experiment, the nature of the experimental manipulation (i.e., how the "graduate student ratings" were actually constructed), and the reactions that were measured. When the debriefing was comcompleted, the experiment was concluded.[3]

### *Measures*

The rating scales completed by the subjects were used in assessing the four dependent variables included in the experiment, each of which was measured by a pre-to-post change score. In the course of the first two sessions, subjects completed the form consisting of 12 rating scales on personal characteristics three times. Actual self-ratings were made twice, once in the initial session as a part of the personality test battery and once in the second session soon after the subject received the bogus ratings. The amount of change in self-rating in the direction of the "graduate student's" evaluations was used as a measure of the subject's *conformity* to the discrepant position. The third time subjects completed the rating form, they were asked to indicate the ratings they had received from the graduate student. The difference between these ratings and the actual ratings given to the subject was taken as a measure of the extent to which the subject *underrecalled* the degree of discrepancy of the received evaluations from his own position. Subjects also completed rating scales dealing with their opinions of the graduate student who evaluated them and with the usefulness and validity of the personality-test information for making judgments about another's personality. Such scales were completed once in the initial session and again after the presentation of the fictitious ratings. The change in a subject's attitude toward the graduate-student rater provided a measure of the *rejection* of the source of the discrepant position. Finally, the change in a subject's evaluations of the usefulness of the personality-test data was used as a measure of the *devaluation* of the validity of the discrepant position.

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[3] A matter of ethical concern arose in that subjects received, in the second ssesion, unfavorable evaluations of themselves supposedly made by the graduate students and yet were not informed of the artificial nature of the evaluations until the third session when the experiment was completed. Although it was considered highly improbable, the possibility existed that a subject, in view of the negative evaluations, would take some action in the week intervening between the second and third sessions that would have unfavorable consequences for himself. Therefore, at the conclusion of the second experimental session, the experimenter read a statement emphasizing that any such action was not warranted on the basis of the ratings. It was explained that highly inaccurate ratings could result from a rater's inexperience, the invalidity of tests, and other reasons. Subjects were told that by the third session more information concerning the accuracy of the ratings would be available and that there was no need to be overly concerned at the present time. They were encouraged, however, to speak to the experimenter if they were concerned about the evaluations they received. (One subject did, in fact, express concern to the experimenter after the second session. The nature of the experiment was immediately explained in full to the subject, and he was excused from the third session.)

In the third session, the dependent measures used in the second session were administered again. To investigate the stability of the reactions over an interval of approximately 1 week, the difference between scores on the dependent measures in the second and third sessions was determined for each dependent variable.

### *Control Group*

Control subjects underwent the same procedures except they did not receive any fictitious ratings. They were told that the graduate students were "in the process of scoring and evaluating the tests" the subjects had taken in the first session. Since no ratings were presented, the control subjects were asked at the beginning of the second session to mark on the self-descriptive scales "the ratings you *expect* that you will receive from the graduate student who rates you." At the end of the session, when experimental subjects recalled the evaluations they received, control subjects were asked to indicate again the ratings they expected to receive. The measure of underrecall for control subjects, then, was the difference between the two sets of expected ratings. Measures of the other dependent variables were obtained in the same manner as that employed with the experimental subjects.

## RESULTS

### *Discrepancy-Level Effects*

Since the dependent variables employed in the present study were change scores, the initial analyses were designed to determine whether there were any differences between the exprimental groups on any of the premeasures used in computing the dependent scores. The premeasures for conformity, rejection, and devaluation were obtained in the first experimental session, and since subjects were arbitrarily assigned to the treatment conditions, significant differences between the groups should not occur. On the other hand, the premeasure for underrecall was the sum of the nine critical items on the rating form presented to the subject at the beginning of the second experimental session (i.e., the form on which the "graduate student" was said to have marked his evaluations). Since the degree of unfavorableness of such ratings was manipulated by the experimenter, there should be large differences between treatment groups on the underecall premeasure. These expectations were supported by the data.

The means on the four dependent variables for each treatment group are presented in Table 1, which also presents the results of analyses of variance performed on each variable. The epsilon square ($\epsilon^2$) is a measure of the strength of the relation between independent and dependent variables (Peters & Van Voorhis, 1940, pp. 353–357).

It can be seen in Table 1 that the discrepancy-level treatments had a significant effect on all four dependent variables. With respect to conformity scores, post hoc tests using the Newman-Keuls method (Winer, 1962) revealed that the moderate and extreme discrepancy groups differed significantly ($p < .05$) from the slight discrepancy and control groups. Mean rejection scores increased markedly with each increase in the unfavorableness of the ratings. The control group, which received no ratings, showed essentially no change in their ratings of the graduate student. Their mean rejection score fell between those for the slight and moderate discrepancy treatments and did not differ significantly from the latter group. All other differences between groups were significant. On devaluation scores similar effects were observed. Devaluation increased significantly with each change in experimental treatment and only the slight discrepancy group failed to differ significantly from the control group. For both rejection and devaluation the $\epsilon^2$ statistic indicates a substantial relationship between independent and dependent variables.

TABLE 1

TREATMENT-GROUP MEANS ON THE FOUR DEPENDENT VARIABLES

| Variable | Discrepancy level | | | | $F$ | $\epsilon^2$ |
|---|---|---|---|---|---|---|
| | Slight | Moderate | Extreme | Control | | |
| Conformity | 0.518 | 7.157 | 7.870 | −0.691 | 9.15** | .097 |
| Rejection | −5.089 | 3.333 | 12.315 | −0.721 | 33.49** | .299 |
| Devaluation | −2.429 | 7.314 | 15.500 | −0.441 | 25.02** | .240 |
| Underrecall | 4.268 | 4.157 | 2.833 | 0.029 | 3.17* | .028 |
| $n$ | 56 | 51 | 54 | 68 | | |

* $p < .05$.
** $p < .001$.

The analysis of variance of underrecall scores also yielded a significant $F$ ratio. The three groups receiving experimental treatments did not differ in the extent to which they underrecalled the graduate student's evaluations, but the slight and moderate discrepancy group means were significantly higher than that of the control group.

Trend analyses for the three treatment groups were conducted for each dependent variable. Significant linear components were found for the conformity, rejection, and devaluation variables, but not for underrecall means. No evidence for curvilinear relationships was obtained in any of the analyses.

Finally, a discriminant analysis of the four treatment groups was performed, using the four dependent measures as variates. The purpose of the discriminant analysis was to determine a set of weights to be used in a linear combination of the variates which would define a new dimension along which the differences among the groups would be maximized (Cooley & Lohnes, 1962). The results of the discriminant analysis are reported in Table 2. The significance of the discriminant function, tested by an approximation of Wilks' lambda coefficient to an $F$ distribution, was well above acceptable levels. The normalized vector presents the weights to be used in determining individual discriminant scores, while the elements of the scaled vector indicate the relative contribution of each of the variates to the discrimination among the groups. A single dimension was found to account for over 90% of the variance. The primary contributors to group separation along the dimension were the rejection and conformity variables, high positive scores on these measures resulting in high negative discriminant scores. Table 3 presents a frequency distribution of discriminant scores for the four groups. The more negative discriminant scores observed as discrepancy level increases are attributable to the higher rejection and conformity scores in these groups.

### *Personality Effects*

A series of analyses was then conducted to determine the effects of the personality vari-

TABLE 2

DISCRIMINANT ANALYSIS OF DISCREPANCY TREATMENT GROUPS

| Variate | Normalized vector | Scaled vector |
|---|---|---|
| Conformity | −.5966 | −98.3716 |
| Rejection | −.7179 | −102.3176 |
| Devaluation | −.2823 | −51.8423 |
| Underrecall | −.2213 | −27.5104 |
| Percentage of trace: 90.87 | | |

TABLE 3

FREQUENCY DISTRIBUTION OF DISCRIMINANT SCORES OF DISCREPANCY TREATMENT GROUPS

| Discriminant scores | Discrepancy treatment group | | | |
|---|---|---|---|---|
| | Slight | Moderate | Extreme | Control |
| 16–23.99 | 4 | 1 | — | 2 |
| 8–15.99 | 12 | 4 | 1 | 16 |
| 0–7.99 | 22 | 2 | 3 | 20 |
| −8–−0.01 | 13 | 11 | 4 | 21 |
| −16–−8.01 | 5 | 23 | 13 | 4 |
| −24–−16.01 | — | 7 | 16 | 5 |
| −32–−24.01 | — | 2 | 11 | — |
| −40–−32.01 | — | — | 6 | — |
| −48–−40.01 | — | 1 | — | — |

ables, both as main effects and in interaction with the discrepancy-level treatments, on the response measures. High, moderate, and low groups on each personality variable were defined by the upper, middle, and lower thirds of the distribution of test scores on that variable. Together with the classification according to which discrepancy-level treatment the subject received, this constituted a 3 × 3 multivariate analysis of variance design, with four dependent variables (Jones, 1966). A series of such analyses was conducted, a different personality variable being employed in each analysis.[4] The variables considered were those measured by the personality questionnaires administered in the first session: the M-C–SDS, the Interpersonal Check List, the *R-S* scale, and the Dogmatism Scale.

*M-C–SDS.* Table 4 presents the means on the dependent variables for the M-C–SDS groups at each discrepancy level.[5] Each cell of the table contains the vector of means for that group on the four dependent variables: conformity, rejection, devaluation, and underrecall. The multivariate analysis of variance indicated that differences associated with the first latent root for interaction effects were highly significant ($p < .01$). When test criteria indicate such significant differences associated with the first discriminant dimension, the possibility exists that there may be a second significant dimension, orthogonal to the first (see Jones, 1966). Further tests were thus conducted, based on the second latent root, to determine whether any significant discrimination could be made after removal of the first dimension. Tests based on the second latent root for interaction effects were also significant ($p < .05$), indicating that the nine groups of the 3 × 3 classification can be maximally discriminated in terms of two discriminant dimensions. (Tests of the third latent root were not significant.)

In view of this significant interaction effect, one-way multivariate analyses of variance were undertaken for the three M-C–SDS groups at each discrepancy-level condition. The resulting *F* ratios indicated that the personality groups did not differ in their response pattern in the slight discrepancy condition, but could be significantly or nearly significantly differentiated in the moderate ($p < .06$) and extreme discrepancy ($p < .05$) conditions. The corresponding discriminant functions showed that in the moderate discrepancy condition the groups were differentiated primarily in terms of their rejection and underrecall scores, while in the extreme discrepancy case devaluation was by far the most important discriminating variate.[6] The discriminant functions were used to determine discriminant scores for each M-C–SDS group, and frequency distributions of the scores are presented in Tables 5 and 6. In Table 5, high positive discriminant scores were determined

TABLE 4

MEANS FOR M-C–SDS GROUPS AT VARIOUS DISCREPANCY LEVELS

| M-C-SDS | Discrepancy level | | | Row *M* |
|---|---|---|---|---|
| | Slight | Moderate | Extreme | |
| High | | | | |
| C | −3.28 | 7.17 | 7.71 | 3.56 |
| R | −6.11 | 6.00 | 13.43 | 3.72 |
| D | −2.78 | 7.61 | 24.36 | 8.56 |
| U | 5.83 | 1.39 | 5.71 | 4.20 |
| | (18) | (18) | (14) | |
| Moderate | | | | |
| C | 1.38 | 5.50 | 4.80 | 3.80 |
| R | −4.10 | −1.83 | 11.20 | 1.78 |
| D | −0.76 | 2.61 | 9.90 | 3.88 |
| U | 4.10 | 9.56 | 2.45 | 5.20 |
| | (21) | (18) | (20) | |
| Low | | | | |
| C | 6.41 | 7.07 | 9.95 | 7.94 |
| R | −5.53 | 5.79 | 12.21 | 4.38 |
| D | −2.71 | 11.71 | 14.05 | 7.70 |
| U | 1.77 | 1.00 | 3.00 | 2.02 |
| | (17) | (14) | (19) | |
| Column *M* | | | | |
| C | 1.41 | 6.54 | 7.42 | 5.03 |
| R | −5.18 | 3.12 | 12.15 | 3.21 |
| D | −2.00 | 6.96 | 15.21 | 6.55 |
| U | 3.95 | 4.22 | 3.51 | 3.89 |

Note.—C, conformity; R, rejection; D, devaluation; U, underrecall. *n*'s are given in parenthesis.

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[4] Since the discrepancy-level effects were presented previously for all subjects, results regarding such main effects in the following analyses are redundant and are not discussed.

[5] Two subjects (one in the moderate discrepancy condition, one in the extreme discrepancy condition) did not complete the Marlowe-Crowne scale.

[6] The elements of the discriminant function for the moderate discrepancy condition were: for conformity, .404; rejection, .739; devaluation, −.020; underrecall, −.711. The values for the extreme discrepancy condition were: for conformity, .503; rejection, −.329; devaluation, 1.155; underrecall, .264.

TABLE 5

DISTRIBUTION OF DISCRIMINANT SCORES FOR M-C–SDS GROUPS AT MODERATE DISCREPANCY LEVEL

| Interval | M-C–SDS groups | | |
|---|---|---|---|
| | High | Moderate | Low |
| 2.50–2.99 | — | — | 1 |
| 2.00–2.49 | 1 | — | — |
| 1.50–1.99 | 2 | 1 | — |
| 1.00–1.49 | 3 | 1 | 2 |
| .50–.99 | 5 | — | 4 |
| .00–.49 | 1 | 3 | 3 |
| −.50–−.01 | 3 | — | 2 |
| −1.00–−.51 | 2 | 7 | 1 |
| −1.50–−1.01 | 1 | 3 | 1 |
| −2.00–−1.51 | — | 1 | — |
| −2.50–−2.01 | — | 2 | — |

primarily by high rejection scores, whereas high underrecall scores led to negative discriminant scores. It is clear that high and low scorers on the M-C–SDS are separated on this dimension from the middle-range group. Comparison of these groups (moderate discrepancy-level column of Table 4) on their mean rejection and underrecall scores (the two heavily-weighted variates) reveals a marked similarity between the high and low groups, while the moderate M-C–SDS group had a quite different pattern of response scores. Similar differences were found in the extreme discrepancy condition as seen in Table 6. Discriminant scores on this dimension were determined largely by devaluation scores, with some weight being given to conformity scores.

TABLE 6

DISTRIBUTION OF DISCRIMINANT SCORES FOR M-C–SDS GROUPS AT EXTREME DISCREPANCY LEVEL

| Interval | M-C–SDS groups | | |
|---|---|---|---|
| | High | Moderate | Low |
| 4.00– | 1 | — | — |
| 3.50–3.99 | 2 | — | — |
| 3.00–3.49 | 2 | — | 1 |
| 2.50–2.99 | — | 1 | 1 |
| 2.00–2.49 | 4 | 1 | 2 |
| 1.50–1.99 | — | 4 | 6 |
| 1.00–1.49 | 1 | 1 | 3 |
| .50–.99 | 4 | 5 | 1 |
| .00–.49 | — | 4 | 5 |
| −.50–−.01 | — | 3 | — |
| −1.00–−.51 | — | 1 | — |

Table 4 (extreme discrepancy-level column) again shows that high and low M-C–SD groups had somewhat similar group means o the variables, while the middle group ha lower scores on both responses.

Finally, univariate analyses of varianc were conducted for each dependent variabl There were no differences on conformity o rejection scores, but for both devaluatio and underrecall scores there were signifi cant interactions between M-C–SDS and th discrepancy-level treatments. Mean devalua tion scores for the M-C–SDS groups at eac discrepancy level are plotted in Figure 1 i order to show graphically the nature of th interaction. While all groups showed an in crease in devaluation as discrepancy leve increased, low M-C–SDS scorers showed a marked increase from the slight to the moder ate discrepancy conditions, whereas high scor ers increased dramatically at the extrem discrepancy level. Subjects with middle-rang scores showed a more gradual increase i devaluation. The interaction for underrecal scores is shown in Figure 2. The high an low M-C–SDS groups tended to decrease an then increase in underrecall as discrepanc level increased, whereas the middle grou showed a marked increase in underrecall i the moderate discrepancy condition an decreased underrecall at the extreme discrepancy level.

The interactions again indicate a tendenc for high and low M-C–SDS scorers to respond

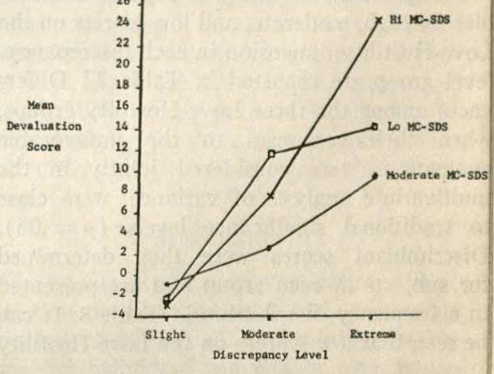

FIG. 1. Mean devaluation scores for M-C–SDS groups at each discrepancy level.

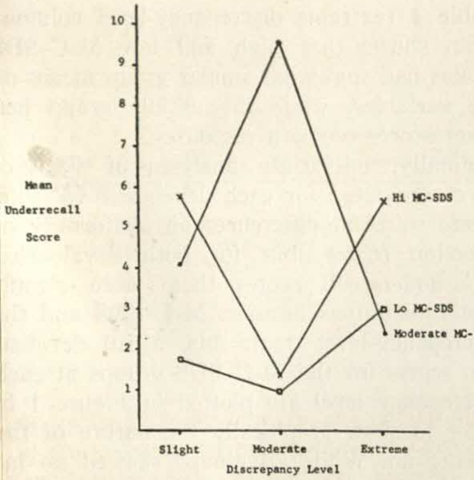

FIG. 2. Mean underrecall scores for MC–SDS groups at each discrepancy level.

similarly, while middle-range scorers show a markedly different pattern of response.

*Interpersonal Check List.* Responses to the Interpersonal Check List (ICL) were scored for the eight octants of the interpersonal domain represented by the ICL items, and scores on the two dimensions of that domain, Dominance-Submission and Love-Hostility, were determined for each subject according to the formulas reported by Leary (1957, p. 69). When subjects were classified according to their scores on the Dominance-Submission dimension, there were no significant effects due to this variable or to its interaction with the discrepancy-level treatments in either the multivariate or univariate analyses of variance.

Group means on the four dependent variables for high, moderate, and low scorers on the Love-Hostility dimension in each discrepancy-level group are reported in Table 7.[7] Differences among the three Love-Hostility groups, when their responses to the unfavorable evaluations were considered jointly in the multivariate analysis of variance, were close to traditional significance levels ($p = .06$). Discriminant scores were then determined for subjects in each group and are presented in a frequency distribution in Table 8. It can be seen that low scorers on the Love-Hostility dimension (i.e., those at the hostility end of the continuum) are separated from the other groups along the discriminant dimension. The elements of the discriminant function indicated that conformity and rejection scores

TABLE 7

MEANS FOR LOVE–HOSTILITY GROUPS AT VARIOUS DISCREPANCY LEVELS

| Love–Hostility | Discrepancy level | | | Row *M* |
|---|---|---|---|---|
| | Slight | Moderate | Extreme | |
| High | | | | |
| C | −0.46 | 6.68 | 4.38 | 3.28 |
| R | −3.05 | 3.53 | 11.94 | 3.35 |
| D | 1.18 | 8.42 | 17.13 | 8.07 |
| U | 4.36 | 7.58 | 3.75 | 5.26 |
| | (22) | (19) | (16) | |
| Moderate | | | | |
| C | −0.19 | 7.33 | 4.90 | 4.00 |
| R | −8.44 | 1.00 | 10.32 | 1.52 |
| D | −6.44 | 2.53 | 16.74 | 5.06 |
| U | 5.63 | 2.73 | 4.79 | 4.44 |
| | (16) | (14) | (19) | |
| Low | | | | |
| C | 3.50 | 7.78 | 12.00 | 7.92 |
| R | −6.38 | 6.11 | 13.11 | 4.69 |
| D | −4.50 | 10.78 | 11.78 | 6.42 |
| U | 3.00 | 0.94 | 0.17 | 1.31 |
| | (16) | (18) | (18) | |
| Column *M* | | | | |
| C | 0.80 | 7.25 | 7.15 | 5.03 |
| R | −5.63 | 3.69 | 11.75 | 3.21 |
| D | −2.76 | 7.54 | 15.17 | 6.59 |
| U | 4.33 | 3.89 | 2.91 | 3.71 |

Note.—C, conformity; R, rejection; D, devaluation; U, underrecall. *n*s are given in parenthesis.

TABLE 8

DISTRIBUTION OF DISCRIMINANT SCORES FOR HIGH, MODERATE, AND LOW LOVE-HOSTILITY GROUPS

| Interval | Love–Hostility groups | | |
|---|---|---|---|
| | High | Moderate | Low |
| 4.00–4.49 | — | — | 1 |
| 3.50–3.99 | — | — | — |
| 3.00–3.49 | — | — | 1 |
| 2.50–2.99 | — | — | 1 |
| 2.00–2.49 | — | 1 | 1 |
| 1.50–1.99 | 2 | 4 | 5 |
| 1.00–1.49 | 7 | 5 | 6 |
| .50–.99 | 6 | 5 | 12 |
| .00–.49 | 10 | 8 | 8 |
| −.50–−.01 | 12 | 8 | 6 |
| −1.00–−.51 | 11 | 5 | 7 |
| −1.50–−1.01 | 5 | 9 | 3 |
| −2.00–−1.51 | 3 | 2 | 1 |
| −2.50–−2.01 | — | 1 | — |
| −3.00–−2.51 | 1 | 1 | — |

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[7] Two subjects (both in the slight discrepancy condition) did not complete the ICL.

TABLE 9

MEANS OF TREATMENT GROUPS ON STABILITY OF RESPONSE MEASURES

| Variable | Discrepancy level group | | | | $F$ | $\epsilon^2$ |
|---|---|---|---|---|---|---|
| | Slight | Moderate | Extreme | Control | | |
| Conformity | 0.234 | −4.304 | −4.437 | −0.578 | 3.64* | .027 |
| Rejection | 2.340 | 1.043 | −1.687 | 0.328 | 3.84** | .040 |
| Devaluation | −0.085 | −0.739 | −2.083 | 0.047 | 0.86 | .000 |
| Underrecall | 2.298 | 2.587 | 5.479 | −1.687 | 6.20*** | .071 |
| $n$ | 47 | 46 | 48 | 64 | | |

* $p < .05$.
** $p < .01$.
*** $p < .001$.

contributed most to the differentiation and were about equally weighted. Underrecall had a slightly smaller, negative weight.[8]

To study the differences further, univariate analyses of variance were conducted for each dependent variable. The only significant effect associated with Love-Hostility was on underrecall scores: the high (love) group had higher underrecall scores than the low (hostility) group, suggesting that subjects who described themselves as cooperative, docile, and affiliative in their interpersonal behavior tended to assimilate the dissonant information towards their own position.

*Other variables.* Similar analyses were performed for high, moderate, and low groups on the Dogmatism Scale and on the *R-S* scale. No significant effects associated with the variables were observed.

### *Stability of Responses to Inconsistency*

In order to examine the stability of response over time, measurements of the dependent variables were taken in a third experimental session. The difference between the subjects' scores from the second to the third sessions was determined for each dependent variable as an index of change in response. Mean change scores on each dependent variable for the four treatment groups are presented in Table 9.

On the change-in-conformity scores, the moderate and extreme discrepancy groups differed significantly from the slight discrepancy and control groups. Subjects in the moderate and extreme discrepancy conditions, who had shown the largest amount of conformity in the second session, evidenced some return to their original position on the self-ratings. However, the extent of the return was not as great as the initial conformity, indicating some net change in the conforming direction.

On the change-in-rejection scores, a significant difference was found between the slight discrepancy subjects, who showed greater rejection in the third session than they had the previous week, and the extreme discrepancy subjects, who were more accepting in their ratings of the graduate student than they had been previously. Both changes represent reversals of initial reactions made in the second session (see Table 1).

Differences among groups in change-in-devaluation scores were not significant, and group means on such change scores were close to zero, indicating little change in the subjects' evaluations of the tests over the week's interval. Devaluation responses thus appeared to be quite stable. On the other hand, subjects in each discrepancy-level condition showed a general *increase* in underrecall scores, while the control subjects demonstrated a slight tendency in the opposite direction. While there were no significant differences among the three experimental groups, each differed significantly from the control group.

## DISCUSSION

The results of the present experiment indicate that both situational and personality variables contribute in determining the response an individual will employ as a means of resolving cognitive inconsistencies. Several points relevant to the interpretations and implications of the findings are briefly discussed here.

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[8] The elements of the discriminant function were: for conformity, .659; rejection, .634; devaluation, −.263; underrecall, −.566.

Analyses of the discrepancy-level effects indicated that mean conformity, rejection, and devaluation scores all increased as discrepancy levels increased. The simultaneous increase of *group means* should not, however, be interpreted as indicating that *individual subjects* employed all three responses to a greater extent at increased discrepancy levels. Indeed, the correlation between conformity and rejection scores was negative in all treatment conditions ($r = -.18$, $-.41$, and $-.41$ for the slight, moderate, and extreme discrepancy groups, respectively, $p < .01$ for the latter two relationships). Hence, the parallel increase in conformity and rejection may be representative of *groups* of subjects, but not of individual response patterns. The negative relationship between the two variables is consistent with the view that the various responses function as alternative means of resolving cognitive inconsistency and that the response employed reflects a personal preference of the individual (Steiner, 1966). On the other hand, more than one response *may* be successfully used by the individual. For example, the correlation between rejection and devaluation scores was significantly positive ($p < .01$) in all three treatment conditions ($r = .53$, $.70$, and $.58$), suggesting that the two responses can easily be used in conjunction with each other in reducing inconsistency.

The results of the discriminant analysis presented in Table 2 indicate that the treatment groups differed primarily in terms of the rejection and conformity responses. It should be noted that devaluation scores were of considerably less importance in differentiating among the treatment groups, even though the analysis of variance of such scores yielded a highly significant $F$ ratio. This is due to the high correlation between rejection and devaluation scores. Because of the latter relationship, devaluation contributed little to the differentiation beyond that contribution already made by the rejection scores.

Results from the analysis of groups differing on M-C–SDS scores revealed strong interactions with the discrepancy level treatments. The findings have implications for the study of the influence of personality variables on response selection. Tables 5 and 6 and Figures 1 and 2 indicate that high and low M-C–SDS groups responded in a similar manner, while middle-range scorers demonstrated a different pattern of response. This finding suggests that the commonly used strategy of comparing high and low groups on a personality variable (especially a comparison of extreme groups) may effectively mask complex relationships. Experimenters must use more than a high low dichotomy of personality variables if such complex relations are to be uncovered. A second implication stems from the difficulty of interpreting the present results. Personality scales, such as the M-C–SDS, are frequently discussed in terms of the person with a *high* score on that scale. Little theoretical consideration is given to characteristics associated with low or intermediate scores. The common assumption is that low scorers are the "opposite" of high scorers, and hence, the high-versus-low group strategy is appropriate. The present findings do not support such an assumption. For this reason, neither the need-for-approval (Crowne & Marlowe, 1964) nor the defensiveness (Ford, 1964; Ford & Hersen, 1967) interpretation of socially desirable responding offers a suitable account of the results. It is clear that the personality dynamics associated with the full range of scores on the Marlowe-Crowne scale need to be investigated.

Dogmatism scores were not related to any response variable in the present study. Theoretical notions (Rokeach, 1960), as well as some recent findings (Gormly, 1968; Powell, 1962), suggest that highly dogmatic subjects, being intolerant of opposing viewpoints, would be more rejecting of the sources of a discrepant position than would nondogmatic subjects. The present results did not confirm the supposition. Previous studies using the Dogmatism Scale had reported inconsistent findings with regard to the relationship of Dogmatism to such responses as conformity (Hunt & Miller, 1968; Miller, 1965; Vaughan, 1964; Vidulich & Kaiman, 1961) and underrecall (Gormly, 1968; Kleck & Wheaton, 1967). The growing number of inconsistent results reported in the literature raises questions concerning the influence of dogmatic thinking on response selection. Perhaps such authoritarian attitudes are more meaningfully related to one's *tolerance* for inconsistency (Miller & Rokeach, 1968) than to individual differences in methods of resolving inconsistencies.

Glass (1968a) has recently developed a theoretical basis for expecting repressors and sensitizers to differ in their response to inconsistency. In the present study, the response measure seemingly most relevant to the Repression-Sensitization dimension is underrecall. This response might be construed as a person's attempt to avoid or repress the amount of inconsistency present at a given time, and hence, repressors might be expected to have higher underrecall scores than sensitizers. Mean differences on underrecall were in the predicted direction, but were not significant ($p = .10$).

The finding that there is a reduction in conformity over time is consistent with previous studies, although it is interesting to note that there was some net conformity in the moderate and extreme discrepancy groups. Whether there would be a further decrease in conformity at a later testing is a question to be examined in future research. With regard to the stability of responses other than conformity, the present study partially replicates the findings of Steiner et al. (1967), despite the different experimental procedures employed in the two studies. In both cases, the primary response mode after a week's interval was underrecall, and in both cases, there was a decrease in the rejection of the source. Mean devaluation scores remained stable in the present study, in contrast to the Steiner et al. finding of a decrease in that response mode. In sum, it appears that, after a time interval, it becomes quite easy to underrecall the unfavorableness of the inconsistent information. This permits the subject to return at least partially to his initial position on the self-ratings and decreases the need for other responses, such as rejecting the source of the adverse information.

The results of the present experiment illustrate the importance of measuring several alternative modes of inconsistency reduction. A person presented with dissonance-arousing information has available several means of restoring consistency, and these may be employed differentially depending upon both individual variables and the social forces influential in the situation.

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# EFFECTS OF EXPECTED DELAY TIME ON THE SUBJECTIVE VALUE OF REWARDS AND PUNISHMENTS[1]

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**Four experiments examined how the length of anticipated temporal delay periods preceding the occurrence of rewards and punishments affects their subjective value for both children and adults. Subjects were confronted with a series of rewards or of punishments which were presented as occurring immediately or after specified delay periods up to several weeks in duration. In each set, subjects indicated their preferences for outcomes of objectively equal value that differed only in the amount of expected time delay before their occurrence. As predicted, as the anticipated delay interval for attainment of a reward increased, the subjective value of the reward decreased for both children and adults. In contrast, the *length* of anticipated delay time did not affect the subjective value of punishments. Any anticipated delay, however, *regardless of duration,* consistently did affect the aversiveness of punishments for adults, but not for children. Adults preferred immediate punishments to more delayed ones, regardless of specific time intervals; children's evaluations of punishments were not influenced systematically by any future temporal considerations. The overall results help clarify how future time may come to guide behavior.**

The subjective experience of time and such phenomena as "time sense," time perception, and time estimation have had enduring interest for psychologists. Much of this research, reviewed by Fraisse (1963) and by Wallace and Rabin (1960), has dealt with temporal experience as a dependent variable. Most of the studies have focused on the development of time concepts, on the experience of the passage or duration of time, and, more recently, on the personality correlates of responses on measures of time perception and time perspective. The anticipated temporal delays that individuals expect before the occurrence of outcomes have more rarely been independent variables in social-psychological research (e.g., Lazarus, 1966; Nomikos, Opton, Averill, & Lazarus, 1968). Yet expectations concerning future time intervals would seem to be extremely important, especially in the regulation of complex choice behavior. In particular, remarkably little is known about how temporal expectations influence human preferences for future rewards and punishments—in spite of the critical role of anticipated future positive and aversive outcomes in self-regulatory processes.

The present experiments therefore examined how the length of anticipated time periods preceding the occurrence of rewards and punishments affects the value attributed to them by the subject and his preferences for them. Thus, anticipated delay time was the independent variable, and its consequences on the individual's assessment of expected rewards and punishments were examined.

The authors' previous research on voluntary delay behavior has been conceptualized in an expectancy-value model, which views each choice as determined by the subjective probability that it will lead to particular consequences and by the subjective value of the consequences (e.g., Mischel, 1966; Rotter, 1954). Additionally, it had been assumed (Mahrer, 1956; Mischel & Metzner, 1962)

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that increasing delay time reduces voluntary delay of reward by decreasing subjective probability for attainment of the delayed reward. Apart from any effects of delay time on subjective probability, delay time may reduce the subjective value of rewards; delayed rewards supposedly are less desirable than immediate gratifications because delay of reward is frustrative (e.g., Renner, 1967). If that is true, then as the anticipated delay time for a reward increases, the subjective value of the reward should decrease; and such an outcome was hypothesized here.

While the effects of time on reward value appear fairly straightforward, the effects of anticipated delay time upon future punishments seem to be far less evident. Data from a few studies (e.g., Cook & Barnes, 1964; Hare, 1966a) have shown that nonpsychopathic adults confronted with a choice between immediate or briefly delayed, but unavoidable, punishments (shock) of the same magnitude overwhelmingly choose the immediate punishment. In such studies the delay periods always have been extremely short, consisting of intervals of only a few seconds (e.g., Cook & Barnes, 1964; Deane, 1961; Elliott, 1966; Hare, 1966b). Apart from the trivially short delay conditions, however, little is known about how anticipated delay time affects the subjective value of punishments. But in most life situations, adaptive human choice behavior requires the individual to cope with alternatives involving lengthy delay periods of days, weeks, and even longer durations, rather than the artificially short intervals favored in previous laboratory research and in animal studies.

Previous studies that did manipulate more substantial anticipated delay periods (Mischel & Grusec, 1967; Mischel & Metzner, 1962) always investigated the effects of time on children's choice among outcomes varying in magnitude (e.g., a smaller reward now or a larger one in 2 weeks). As expected, fewer larger delayed rewards (as opposed to immediate smaller ones) were chosen by children when the anticipated delay time increased. Surprisingly, the length of the anticipated temporal delay period did not influence choice among punishments that differed in magnitude, although similar anticipated temporal durations did affect reward preferences markedly (Mischel & Grusec, 1967). The results leave one uncertain about the role of anticipated time on value (especially in the case of punishments) because the delayed and immediate outcome were never of the same magnitude and also because the effect of time on reward choice might have been mediated by changes in subjective probability rather than subjective value.

A systematic test of the role of anticipated time on value would require obtaining indexes of attractiveness or preference for outcomes of equivalent magnitude or objective value, realistically presented as occurring after anticipated delay periods of different length. Moreover, since more delayed future events are apt to be perceived as less likely to occur than sooner ones, it would be necessary to arrange the situation in a way that minimized the role of probability of occurrence. Because the effects of anticipated time on value may involve developmental changes, it also would be of interest to sample the behavior of both children and adults in this regard. Finally, because preference for delayed rewards and delayed punishments may be entirely unrelated (Grusec, 1968; Mischel & Grusec, 1967), it is necessary to study the effect of anticipated time on the evaluation of positive and aversive events separately.

To achieve the foregoing objectives, subjects were confronted with a series of rewards or punishments and, to enhance realistic behavior, were led to believe that some of the outcomes would really occur. The same sets of rewards were presented as occurring immediately or after specified delay periods. Similarly, the same sets of punishments were presented as occurring immediately or after specified delay periods. Subjects rated the relative value (attractiveness or unpleasantness) of the rewarding and punishing outcomes occurring immediately or at different temporal delays. The procedure was used in two separate experiments, one with children (Experiment I) and the other with adults (Experiment III). In addition, in Experiment II the children rated the subjective value of delayed rewards and punishments under conditions in which all outcomes were delayed, but the length of delay time was manipulated. Finally, adult subjects were asked to show their preferences among sets of inevitable punishments (electric shocks) of the same magnitude available after different time intervals (Experiment IV). The last experiment also explored the role of punishment intensity, and of psychometrically assessed anxiety, in preferences for immediate or more delayed aversive outcomes.

## EXPERIMENT I

### *Method*

*Subjects*

The subjects in the first experiment were 16 boys and 20 girls, of middle-class socioeconomic status, from the fourth and fifth grades of a Mountain View, California, elementary school.

*Design*

All children rated the relative value (attractiveness) of four immediate rewards and of the same rewards when they could be obtained only after specific delay intervals of 1 day, 1 week, and 3 weeks. Similarly, they also rated the value, in terms of relative unpleasantness, of four immediate negative outcomes (punishments) and of the same punishments when they would occur after delay intervals of 1 day, 1 week, and 3 weeks. The delay intervals for all items were presented in counterbalanced order (from 1 day to 1 week to 3 weeks, and from 3 weeks to 1 week to 1 day). Subjects were randomly assigned to one of two rating sequences—a group in which children made reward-value ratings first (Condition I), and a group in which children made punishment-value ratings first (Condition II). Analysis of the data included a test of the effects of this sequence.

*Procedure*

The children were tested in small groups by a female experimenter in a mobile laboratory parked in the school yard. They were given booklets which included a brief description of each item they were being asked to rate.

*Condition I.* Subjects in Condition I were told that they each would obtain a prize, either that day or later—the next day, in 1 week, or in 3 weeks. The experimenter then wrote the particular prize and the time at which it would be received on a slip of paper, which she folded so that the writing could not be seen and pinned to the wall. She said that although the prize and time were definitely selected and unchangeable, subjects would find out which prize they would get and when they would get it only after they were finished in the trailer. These preliminary instructions were employed with all subjects to create a plausible situation in which outcomes would occur actually rather than hypothetically. Thus, their value could be rated realistically, while it was also guaranteed that the ratings could not determine the outcomes.

The experimenter then displayed, one at a time, four rewards—50¢, a notepad, six packs of chewing gum, and 80¢—and described each as available immediately or at one later time. For example, she described 50¢ now or 50¢ at a later specified time of either 1 day, 1 week, or 3 weeks. The children were asked to indicate, in their printed booklets, whether they thought the immediate reward or the same reward but at the later time was more attractive, or whether the two were equally attractive, by placing a mark beside the appropriate printed statement in their booklets. It was emphasized that there were no right or wrong answers, that the prize which they would get was already determined and did not depend on their answers, and that the important thing was simply indicating what they "really felt" in response to each item.

Subjects recorded their ratings in their booklets before the experimenter proceeded to the next reward. On each page of the booklet all four rewards were presented, and each was shown as available at two delay intervals, for example, immediately or the next day. On each page the children rated the four reward items. For each reward, the rating required the child to indicate whether the reward was more attractive when it was available at the earlier time, or at the later time, or whether it was equally attractive at both times. The procedure was repeated until all the rewards were presented and rated at all the paired delay-time combinations sampled. For half the subjects, the delay intervals were presented in ascending sequence (from 1 day to 1 week to 3 weeks), while for the other half, they were presented in the reverse order.

As soon as all the rewards had been rated, the children were told that they would have to do something unpleasant, again either that day or after a specific delay time. The experimenter wrote the unpleasant outcome and the time at which it would occur on a slip of paper which she pinned to the wall and covered so that it could not be seen. The subjects then rated the relative unpleasantness of four punishments occurring at different delay times. Their booklets contained brief descriptions of each punishment, and the children indicated whether they thought having to do the unpleasant thing immediately or at specified later times was more unpleasant. If they thought the punishment was equally unpleasant regardless of delay interval, they could indicate so. Again it was emphasized that there were no right or wrong answers, that all answers were equally good, and that the particular negative outcome that really would occur had already been selected.

The four punishments were: Eat a can of bad-tasting food; listen to a noise which hurts your ears for 15 minutes; spend 30 minutes with an unpleasant teacher; and do four difficult tasks in front of the class. The items and their aversive features were elaborated further by the experimenter as she described each one in detail. The administration was identical to that used for the reward items.

*Condition II.* In Condition II the punishments

TABLE 1

MEAN VALUE RATINGS OF MORE IMMEDIATE OUTCOMES WHEN PAIRED WITH THE SAME OUTCOMES INVOLVING GREATER DELAY

| Delay time (days) | | Mean value | | | | | |
|---|---|---|---|---|---|---|---|
| | | Rewards[a] | | | Punishments[b] | | |
| Exp. I & III | Exp. II | Exp. I (Children) | Exp.II (Children) | Exp. III (Adults) | Exp.I (Children) | Exp. II (Children) | Exp. III (Adults) |
| 0 vs. 1 | 0 vs. 1 | .61 | 1.22 | 1.55 | .36 | −.31 | .75 |
| 0 vs. 7 | 1 vs. 7 | 1.08 | 1.36 | 1.85 | −.05 | −.31 | .90 |
| 0 vs. 21 | 7 vs. 21 | 1.47 | 2.06 | 2.10 | −.39 | −.36 | .80 |

[a] Positive scores indicate that more immediate rewards are more attractive.

[b] Positive scores indicate that more immediate punishments are less aversive.

were administered first, followed by the rewards. Otherwise, the procedure was the same as in Condition I. Subjects were not told about the second set of ratings they would have to make in either Conditions I or II until they had completed the first set.

At the end of the experimental session, all subjects were told which reward they would get and when they would get it and were also informed that they would not have to do any of the unpleasant things. In addition, they were asked not to discuss any part of the experimental procedure with their fellow students. The importance of this, and the reasons for it, were explained.

### *Scoring*

For each reward rating, subjects were assigned a score of +1 if they rated the immediate reward as more attractive, −1 if they rated the delayed reward as more attractive, and 0 if they rated them as equally attractive. Likewise, for each punishment rating, they were given a score of +1 if they rated the immediate punishment as less unpleasant, −1 if they rated the delayed punishment as less unpleasant, and a score of 0 if they rated both equally unpleasant. The data provided value scores for four rewards and four punishments, each rated for three anticipated delay intervals. Each subject's value scores for each anticipated delay period could thus range from +4 to −4. At each delay interval, a subject who obtained a positive score for rewards tended to rate immediate rewards as more attractive than delayed ones, while a subject who obtained a positive score for punishments tended to rate immediate punishments as less aversive than delayed punishments.

## *Results*

There were no appreciable differences in the ratings made by boys and girls, and therefore their data were combined for the statistical analyses. Table 1 presents the mean value-rating scores for rewards and punishments in all conditions (see columns labeled Experiment I).

### *Rewards*

Analysis of variance on the value ratings of rewards at the three expected delay times yielded a significant effect of delay time ($F = 4.32$; $df = 2/68$; $p < .05$). Rating sequence (whether rewards or punishments were rated first) did not have a significant effect on ratings ($F = .42$), nor was there a significant interaction between delay time and rating-sequence ($F = 1.37$). Comparisons by $t$ tests between the three delay times are presented in Table 2.

While only the difference between value ratings at 1 day and 3 weeks reached significance, the other differences were in the predicted direction. Thus, as the time interval associated with the delayed reward increased, subjects were more inclined to rate the immediate reward as more attractive.

In addition, the study examined the effect

TABLE 2

$t$ VALUES FOR COMPARISONS OF MEAN VALUE RATINGS BETWEEN DELAY TIMES (EXPERIMENTS I AND III)

| Delay time (days) | Rewards | | Punishments | |
|---|---|---|---|---|
| | Exp. I (Children) | Exp. III (Adults) | Exp. I (Children) | Exp. III (Adults) |
| 1 vs. 7 | 1.60 | 1.58 | *ns* | *ns* |
| 7 vs. 21 | 1.33 | 1.32 | *ns* | *ns* |
| 1 vs. 21 | 2.93* | 2.89* | *ns* | *ns* |

* $p < .005$.

of anticipated delay, regardless of length of temporal interval, on the subjective value of rewards. Recall that a score of 0 was assigned when outcomes were rated as equally attractive, which means that if subjects indicated immediate and delayed rewards were equally attractive, their total scores should not differ significantly from 0. Moreover, if they rated a delayed reward as more attractive (obtaining a score of $-1$) approximately half the time, and as less attractive (obtaining a score of $+1$) approximately half the time, their total scores again should not differ from 0. Thus separate $t$ tests were done at each delay level to determine if the mean scores did in fact differ significantly from 0.[3] The $t$-test comparisons, presented in Table 3, show that at all delay levels subjects rated the immediate rewards as significantly more attractive than the delayed rewards.

### *Punishments*

Analysis of variance done on the value ratings of punishments yielded no significant main effects ($F = 1.94$ for effects of delay time and $F = .27$ for effects of rating sequence). The interaction between delay time and rating sequence ($F = .60$) also was not significant.

Since there were no differences between the three delay periods, the mean value ratings of items at all time intervals combined was compared with zero. As can be seen from Table 3, children did not rate immediate punishments as significantly different in value from delayed punishments.

## EXPERIMENT II

The first experiment showed that anticipated delay time affects children's value ratings of rewards, but not of punishments, when judgments require comparisons between immediately available and delayed outcomes. Moreover, at each delay interval immediate rewards were judged more valuable, but ratings of immediate punishments did not differ from 0. It would also be interesting to study the effects on value ratings when the subject must judge the relative attractiveness of the same two rewards if *both* require delay, but one requires more delay than the other. Likewise, does anticipated delay time affect value ratings of punishments if subjects rate the relative aversiveness of two punishments when both involve delay, but one is more delayed than the other? The second experiment was designed to explore these questions and to clarify further the effects of anticipated delay on the subjective value of outcomes.

### *Method*

The subjects were 18 boys and 18 girls from the same grade levels and school as those in the first experiment, but who had not participated in the initial experiment. All measures and procedures were identical to those in the first experiment except that the value ratings were between outcomes obtainable after 1 versus 7 days, after 7 versus 21 days, and, as a partial replication of the first experiment, between outcomes occurring immediately (on the same day) versus after a 1-day delay.

### *Results*

The only significant $F$ was for the effect of delay time on the value of rewards ($F = 3.64$; $df = 2/68$; $p < .05$). Table 1 presents

TABLE 3

$t$ VALUES FOR COMPARISONS OF MEAN VALUE RATINGS WITH ZERO

| Delay time (days) | | Rewards | | | Punishments | | |
|---|---|---|---|---|---|---|---|
| Exp. I & III | Exp. II | Exp. I (Children) | Exp. II (Children) | Exp. III (Adults) | Exp. I[a] (Children | Exp. II (Children) | Exp. III[a] (Adults) |
| 0 vs. 1 | 0 vs. 1 | 2.51** | 3.81*** | 4.31*** | .12 | 1.55 | 1.79* |
| 0 vs. 7 | 1 vs. 7 | 2.76*** | 2.58*** | 4.51*** | | .89 | |
| 0 vs. 21 | 7 vs. 21 | 6.65*** | 6.87*** | 4.47*** | | 1.03 | |

[a] All delay times combined.
* $p < .05$.
** $p < .01$.
*** $p < .005$.

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[3] $p$ values are for one-tailed $t$ tests.

the relevant mean value-rating scores for rewards and punishments under the columns labeled Experiment II. Separate *t* tests were done to compare the mean ratings with 0 for each of the three temporal combinations. The results are shown in Table 3. As the columns for Experimental II reveal, subjects consistently rated earlier rewards more attractive than delayed rewards, even when both rewards required some delay. Again, anticipated delay time did not affect the rated aversiveness of punishments.

## EXPERIMENT III

The third experiment was designed to assess the generality of the effects found in the first experiment by employing similar procedures with a sample of adult subjects. This permits us to see whether the obtained effects of delay time on the value of rewards, and the independence of delay time and the subjective aversiveness of punishments, consistently found for children, also extends to adults.

### *Method*

#### *Subjects*

The subjects were 10 men and 10 women enrolled in an introductory psychology course at Stanford University. The experiment reported here was one of a number for which they could volunteer in order to fulfill a course requirement.

#### *Procedure*

The procedure followed was identical to that of Experiment I, with the following exceptions. Subjects were tested in an experimental room in groups of five, with either three men and two women or two men and three women in each group. When subjects had originally volunteered for the experiment, they had been told that they would have to return at some later time in order to receive full credit for the experiment. After the introductory instructions had been administered and just as subjects were about to rate rewards (Condition I) or punishments (Condition II), they were reminded that they would have to return and were told that it would be either the next day, in 1 week, or in 3 weeks. The return would be necessary even if they received the reward or punishment that day, and they would be told the exact time at the end of the experimental session. This particular procedure was employed so that subjects' ratings of value would not be influenced by the aversiveness of having to return at a later time. For example, a delayed reward might be less attractive than an immediate one, not only because one had to wait to receive it, but also because one would have to go to the trouble of arranging to be at a certain place at a certain time in order to receive it. The problem did not arise in Experiment I, since the children were told that the experimenter would return to them in their classrooms.

The rewards and punishments were altered so as to make them appropriate for adults. Thus the rewards consisted of: $1, a marking pen, a Cozy Lite reading lamp, and $2. The punishments were: Eat a can of bad-tasting food; experience an unpleasant electric shock; drink some very bitter liquid; and experience a "cold pressor" test.

### *Results*

Inspection of the data revealed no difference between the responses of men and women, so their ratings were combined for purposes of analysis. Table 1 presents the mean value-rating scores for rewards and punishments in all conditions.

#### *Rewards*

Analysis of variance of the value ratings of rewards showed a significant effect of delay time ($F = 4.19$; $df = 2/36$; $p < .05$). Value ratings were not affected by rating sequence ($F = 1.31$), nor was there a significant interaction between delay time and rating sequence ($F = 2.83$). Comparisons by *t* tests between the three delay times are shown in Table 2. Again, while only the difference in value ratings between 1 day and 3 weeks reached significance, the other differences were in the predicted direction. That is, as the delay interval associated with a delayed reward increased, subjects rated the immediate reward as more attractive.

Separate *t* tests, done at each delay level, were calculated to see if mean scores differed significantly from 0, and the results are presented in Table 3. Just as in the data for children, adult subjects at all delay levels rated the immediate rewards as significantly more attractive than the delayed rewards.

#### *Punishments*

As with the data from the children, analysis of variance on the value ratings of punishments did not yield a significant effect of delay time ($F = .23$), nor of rating sequence ($F = 2.68$), and again there was no significant interaction between delay time and rating sequence ($F = .23$). The mean value ratings of items at all delay times were averaged to see if they differed from 0. It is clear from Table

3 that, unlike children, adults rated immediate punishments as significantly less unpleasant than delayed punishments, although the effect was not strong ($t = 1.79$; $p < .05$, one-tailed).

In summary, increasing delay time affected the value of rewards and punishments for adults in exactly the same manner that it did for children: as the delay time associated with rewards increased, their rated subjective value decreased. In contrast, the aversiveness of delayed punishments was unaffected by increasing intervals of delay time. Both children and adults rated immediate rewards as more attractive than delayed rewards. Only children rated immediate punishments as no more or no less attractive than delayed punishments. Adults, on the other hand, regardless of length of delay interval, tended to judge punishments as less aversive when they were immediate than when they were delayed. The tendency, however, was not very pronounced, and firm conclusions were hard to reach in view of the small sample of adults tested. Therefore, a fourth experiment was designed to assess more fully the effects of anticipated delay time on preference among punishments in adult subjects.

## EXPERIMENT IV

The purpose of the fourth experiment was to investigate the effect of substantial anticipated delay intervals on adults' preferences for unavoidable aversive outcomes or punishments. Rather than using value ratings, the present experiment assessed value more directly by eliciting the subjects' realistic choice preferences. The main question was whether immediate punishments are preferred to similar, but delayed, unavoidable punishments of equivalent magnitude (electric shocks). Moreover, if such preferences occur systematically, are they affected by the expected length of the delay period? Experiment IV, in addition, manipulated the intensity of the expected punishments in order to determine how the magnitude of anticipated punishments influences the desire to postpone them or, instead, to terminate them by "getting them out of the way" sooner. Finally, the relations between individual differences in psychometrically measured anxiety and the subjects' preferences for more immediate or more delayed unavoidable punishments were assessed.

### *Method*

#### *Subjects*

The subjects were college undergraduates, 26 men and 25 women, who had been recruited from a psychology course to participate for class credit. From the outset, the subjects signed up in class for the study with the understanding that a commitment to 3 separate hours would be required.

#### *Procedure*

When he arrived in the experimental room for the first session, each subject was told he was participating in an experiment which would definitely involve receiving electric shock in one session and answering attitude and interest measures in two sessions. The rationale given for administering electric shock was the measurement of reactions to pain. To create a realistic atmosphere, the experimenter was assisted by a "technician," who wore a white laboratory coat containing a stethoscope plainly protruding from a side pocket. Moreover, each subject was seen in a laboratory room containing impressive-looking equipment for the physiological measurement of autonomic reactions. The paraphernalia was intended to enhance the plausibility of asking subjects to undergo shock and other assessments and to further strengthen the belief that the shock would actually be administered.

At the start, each subject was given the opportunity to decline to participate or to commit himself to continuing for the three sessions. Only one subject declined to participate. Following this introduction, subjects were asked to schedule the particular times at which they could come to the two later sessions. The design required that subjects would expect to have to return at specific later times so that they could plausibly be requested to choose to undergo electric shocks either at sooner or at later times. So that a reasonably broad set of delay combinations could be sampled, half the subjects (Group 1) were told that the meetings were to be the current one, one the next day, and one a week away. The other subjects were assigned to Group 2, in which the meetings would be the current one, one a week away, and one 3 weeks hence, providing more extended delay periods. Subjects were instructed to list the times they would be able to come on the appointed days, and appointment cards were handed out to increase their expectation that they would indeed be expected to return. In addition, they were reminded that they would lose credit if they failed to attend either of the subsequent meetings. The previous procedure was intended to insure that the students really believed that the future meetings would indeed occur and were unavoidable.

The electric shock was described at this point in the experiment. Subjects in one group were told that it would be "a relatively low electric shock, of approximately 1 second duration, considered a relatively mild electric shock." Subjects in the second group were told that it would be "a rather high electric shock, of approximately 3 seconds duration, considered a relatively severe electric shock." To obtain a measure of the subjective intensity of the proposed shock, all subjects were then told that the experimenters were interested in personal, subjective expectations, and subjects were asked to rate how they expected the shock to feel. They made the ratings on a 4-point scale ranging from "not pleasant," through "mildly" and "moderately unpleasant," to "quite unpleasant."

### *Preferences among Delayed Punishments*

After the subjective intensity scale had been completed, the students were asked to select the times at which they would prefer to have the shocks administered: at that experimental session (i.e., immediately) or when they returned for either the first or the second attitude and interest survey. The delay combinations available to the subject depended on the future meeting times of the two additional sessions for which he had been scheduled. Depending on the condition to which they had been assigned earlier, approximately half the subjects had to choose between the shock now or tomorrow and also between the shock tomorrow or in 1 week. The remaining subjects were asked to choose between the shock now or in 3 weeks and also between shock in 1 week or in 3 weeks. In addition, all students were asked to choose between the shock now or the shock 1 week later. Thus each subject made three choices. The choices for each subject were presented to him in random sequence and in the form of printed booklets.

Subjects were asked to indicate their preference in each choice set realistically because *"one* of the preferences you express in this inventory will actually be administered," although subjects would not be told the particular one until the end. They were given a mimeographed "choice-preference inventory" in which to express in writing their preference for the date of shock administration. The inventory contained three pages, listing all possible pairings of the selected delay intervals. For example:

> Which of the following would you prefer? Circle either A or B.
>
> A. The previously described shock now.
>
> B. The previously described shock in 1 week.

The order of the three choices and the order of listing of the preferences within each item were randomized across all subjects by using different printed booklets. The foregoing choice combinations thus made it possible to sample realistically preferences for shocks of equal magnitude expected after varying temporal delays.

After they had completed the choice-preference inventory, subjects were informed of the nature of the experiment and were debriefed in detail. They were told that they would not have to experience an electric shock and that they could disregard one of their future appointments. However, they were asked to keep the appointment in 1 week's time, since there was indeed a survey being conducted, and they would have to fill out a questionnaire which would take approximately 10 minutes. When they returned for the second appointment, they were given a copy of the Taylor Manifest Anxiety (*MA*) scale (not being told what it was) to answer in written form.

### *Results*

Subjects who were told the shock would be strong tended to rate it as more unpleasant than did those who were told to expect a mild electric shock ($F = 25.21$; $df = 1/47$, $p < .001$). Thus, the experimental manipulation of expectancy concerning the intensity of the anticipated shock was effective. There was no sex difference in reported subjective expectancies ($F < 1$).

Inspection of the delay-preference data revealed no systematic differences in choices as a function of the length of delay period involved in each choice pair (see Table 4). There was no consistent sex difference ($F < 1$; $df = 1/47$), nor was there a significant tendency for subjects told the shock would be strong to select the earlier time of administration more often than subjects led to expect a low-level shock ($F < 1$). The distribution of subjective ratings of expected shock intensity also was dichotomized into high and low halves. Students who reported the highest subjectively expected shock levels did not differ appreciably in their choices from those who rated the expected shocks as less severe, and there was no sex difference.

Subjective expectancy concerning the unpleasantness of the shock was uncorrelated with preference for the earlier administration of shock ($r = .06$). The correlation is probably attenuated since the range was severely restricted for both variables (0–3 for administration preferences, 0–4 for subjective intensity). Scores on the *MA* scale were essentially uncorrelated with either subjective expectancy of shock intensity ($r = .09$) or preference for early administration of shock ($r = -.16$).[4]

The main question of the experiment was whether subjects chose to experience unavoidable punishments sooner or later, regardless of the specific delay periods. Table 4 shows

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[4] A recent study by Hodges (1968) has obtained some relations between indicators of dispositional anxiety and situational reactions to stress.

TABLE 4

CHOICE OF DELAYED SHOCK IN EACH CHOICE PAIR

| Choice | $N$ | $N$ choosing delay | $z$[a] |
|---|---|---|---|
| (Group 1) | | | |
| Now–tomorrow | 25 | 4 | 3.40*** |
| Now–1 wk. | 25 | 5 | 3.00** |
| Tomorrow–1 wk. | 25 | 2 | 4.20*** |
| (Group 2) | | | |
| Now–1 wk. | 26 | 4 | 3.50*** |
| Now–3 wk. | 26 | 3 | 4.90*** |
| 1 wk–3 wk. | 26 | 6 | 2.70* |

[a] To test that the observed percentage choosing delayed shock is less than 50%, the percentage expected by chance.
* $p < .01$.
** $p < .005$.
*** $p < .001$.

the number of subjects in each choice combination who chose the more delayed time for undergoing the shock. In each choice combination the percentage of delayed choices was significantly less than the 50% that would be expected by chance, all $p$ values being less than .01.

In sum, the overall results indicate that when given control over the time of administration of an aversive stimulus or punishment, adults overwhelmingly preferred the sooner of two times in choice situations. Moreover, the subjects tended to choose the sooner time regardless of the length of the specific alternative times involved in the choice, and regardless of the expected intensity of the noxious event.

## DISCUSSION: EXPERIMENTS I, II, III, AND IV

The results of the first three experiments strongly support the hypothesis that as the anticipated delay interval for attainment of a reward increases, the subjective value of the reward decreases. The relationship appears to be linear: the greater the length of anticipated delay, the lower the subjective value of the reward. This finding was revealed by comparing the value ratings obtained when the amount of anticipated delay time varied from relatively shorter to longer durations. Moreover, the findings held for both children and adults.

Anticipated delay decreased the rated value of a reward even when subjects compared the value of two rewards both of which required delay, but with one entailing even more delay than the other. At each delay level, the more immediately available reward was rated more valuable than the one requiring greater delay, further documenting the potent effects of time on reward value.

The findings that length of anticipated delay time decreases the subjective value of rewards help to clarify the previously obtained results that larger delayed rewards are chosen more than smaller immediate ones when the delay time is decreased (Mischel & Grusec, 1967; Mischel & Metzner, 1962). The present results support the view that the effect of time on choice seems to be mediated in part by decrements in the subjective value of rewards as a function of the anticipated duration of delay time. In addition, it is of course possible that in many contexts delay time may also decrease subjective probability, more distant events being perceived as less likely to materialize. Thus, anticipated delay time may affect both the value and probability of rewards, and its effects on voluntary delay choices for rewards may be mediated by changes in both variables. Since subjective value and subjective probability are not necessarily independent (e.g., Mischel & Masters, 1966), it may be very difficult to untangle the exact role of each variable.

The effect of *anticipated* delay time on reward value should not be confused with the possible effects of the actual time a subject is forced to wait for a reward on his subsequent ratings of the reward's value. For example, consistent with dissonance theory (Festinger, 1957), one would expect subjects to rate rewards as *more* valuable the longer the time interval during which they actually had to wait for them, if ratings are made *after* the waiting period by subjects who chose to wait.

While anticipated delay time significantly affected judgments of reward value in both children and adults, anticipated delay before the occurrence of punishments did not influence childrens' evaluations of the aversiveness of the punishments. Adults, in contrast, preferred sooner aversive outcomes to later ones of equal magnitude. Even with adults, however, the *amount* of temporal delay did not significantly influence reactions to the anticipated punishment. Thus, adults rated immediate punishments as generally less aversive than delayed ones of the same intensity (Experiment III) and preferred them overwhelmingly (Experiment IV), irrespective of particular delay lengths. The findings, based on lengthy delay intervals of days and even weeks, are remarkably congruent with results obtained for short temporal delays involving seconds (e.g., Cook & Barnes, 1964; Hare, 1966a, 1966b). With time intervals of less than a minute, it has been shown that adult nonpsychopaths choose predominantly short, rather than longer, delays of inevitable shock. Evidence from such studies and the present investigation suggests that for normal adults, delayed punishments, regardless of length of delay time, are more aversive than the same punishment immediately. In general, preference for the more immediate punishment (shock) in choice situations may be so strong that other variables, such as psychometric anxiety, punishment intensity, and length of delay time (whether seconds, or days and weeks as in Experiment IV), tend to exert little or no influence (e.g., Hare, 1966a; Hare, Krebs, Creighton, & Petrusic, 1966).

The repeated finding that normal adults prefer sooner to later unavoidable punishments supports the hypothesis that waiting for an aversive outcome is itself aversive and adds to the negative valence of the punishment. If waiting for a punishment is unpleasant, one would also expect that longer waiting times should be increasingly aversive in a linear manner: the longer the delay the more subjectively aversive the punishment becomes. This expectation now seems to be consistently disconfirmed: while normal adults prefer sooner unavoidable punishments to later ones, that preference seems to be relatively unaffected by the length of the expected delay periods, whether it is seconds (Cook & Barnes, 1964; Hare, 1966a) or days and weeks as in the present study.

The relationship between expected delay time and subjective aversiveness of the punishment thus does not appear to be linear. In part this finding may be due to the strong preference for the sooner punishment which may override or prevent effects from other variables, such as duration of expected delay. While that interpretation is plausible in Experiment IV, it does not seem reasonable in Experiment III, in which value ratings favoring the sooner punishment (irrespective of particular delay interval) were by no means overwhelmingly strong. It may therefore be necessary to speculate about mediating conditions such as anxiety arousal that could neutralize the role of length of expected delay time by producing less subtly discriminating judgments. Although the mediating mechanism is uncertain, it seems plain that anticipated time does not have exactly the same effect on the evaluation of aversive outcomes as it does on the evaluation of positively valenced events.

For children, delayed punishments apparently do not differ in aversiveness from the same punishments which have to be experienced immediately. The latter result is also in accord with the recent finding that children choose unavoidable delayed punishments about as often as they choose immediate punishments of the same magnitude (Grusec, 1968). Thus, while nonpsychopathic adults rate more immediate punishments as less aversive than delayed punishments of the same magnitude and choose them more often in realistic binary choices, normal children (and adult psychopaths, see Hare, 1966b) do not show such systematic preferences. Presumably between the ages of about 10 and 20—the ages of the subjects participating in the present experiments—one normally learns to anticipate more fully the aversiveness of future punishments and hence to dread and shun them more strongly.

Alternatively, it might be argued that the children's failure to consistently prefer immediate punishments to more delayed ones might reflect that they were not as certain subjectively that the delayed punishment would actually occur. While that argument might be reasonable when subjects are asked to choose between outcomes (Grusec, 1968; Mischel & Grusec, 1967), it does not seem plausible when subjects simply rate the attractiveness of predetermined outcomes (as in the present Experiments I and II with children) rather than choosing among them. It also might be tempting to argue that children were unaffected in their judgments of aversive outcomes by anticipated time because they have not yet developed realistic concepts about future time. That interpretation seems to be ruled out, however, by the fact that the identical units of anticipated time appreciably affected the same childrens' assessments of the value of positive outcomes. The exact processes through which people ultimately come to prefer sooner inevitable aversive outcomes to more delayed, albeit objectively equal, punishments merit close examination. Such preferences seem a critical part of adaptive future-oriented choice behavior, although their determinants have only begun to receive systematic research attention.

Both social learning and cognitive formulations of goal-directed behavior assign a prominent theoretical place to the individual's expectations about future outcomes of either positive or negative valence. Given the theoretical importance of such expectations in the regulation of behavior, an adequate understanding of their temporal features seems especially critical. The present findings, by showing that anticipated temporal delays may significantly modify the subjective valence of future outcomes, point to one of the processes through which, in the course of development, expected future time may become a guide for choice behavior.

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# INFLUENCE OF THE REPRESSION-SENSITIZATION DIMENSION, AFFECT VALUE, AND EGO THREAT ON INCIDENTAL LEARNING[1]

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As they attempted to learn a sequentially presented list of 12 trigrams, represser and sensitizer groups were incidentally exposed to an equal number of English words of either all neutral, positive, or negative affect value. The hypothesis was supported that under high ego threat, sensitizers show a general increment in incidental learning, while repressers show a general decrement in learning. That the greatest amount of differential defensive behavior was not shown for the negative affect words was interpreted as suggesting that sensitizers may have employed projective defenses. As predicted, overall learning of the English words was in the order: positive, negative, neutral.

In recent explorations of the relationships between defensive behavior and cognitive performance, two distinct defensive modes have emerged: Repressers respond to threat in an avoiding, denying manner, and sensitizers respond in an approaching, vigilant way (Byrne, 1965). Viewing defensive behavior as a specific learned pattern of response to a threatening stimulus, the present research was designed to investigate the relationship between the represser-sensitizer distinction and incidental learning. Incidental learning is learning which occurs when a subject neither is prepared by experimental instructions for a subsequent retention test nor gives any subsequent evidence of an "intention" to learn the material (Postman, 1964).

While experimental evidence for the significance of defensive behavior on cognitive performance continues to accrue, there exists a confusion due to a multitude of instruments being used to assess an individual's position on the repression-sensitization continuum (Eriksen, 1966). Confounding matters is the finding that the questionnaire-type Byrne Repression-Sensitization (*R–S*) scale (Byrne, Barry, & Nelson, 1963) used in the present study does not correlate with another popular instrument, the Rorschach Index of Repressive Style (RIRS—Levine & Spivak, 1964). This would suggest that the two scales are not quite measuring the same phenomenon and that additional construct-validity work is needed to clarify the situation.

Eriksen (1966) found that repressers showed a relative difficulty in the intentional learning of threat-related stimuli; for sensitizers there was little difference in learning between threat-related and neutral stimuli. Of greater relevance for incidental learning, Luborsky, Blinder, and Schimek (1965) found different eye-movement patterns for repressers and sensitizers: "the defense of isolation [i.e., sensitization] is associated with venturing to look around more . . . repression with looking around less [Luborsky et al., 1965, p. 278]." Based on some earlier research (McLaughlin, 1965) it seems likely that incidental learning would be especially sensitive, possibly more suitable than an intentional learning task, to such difference in attention-deployment patterns.

Opposing the suggestion that differences in incidental learning may ensue as a function of how a subject copes with anxiety is Easterbrook's (1959) cue-utilization hypothesis. The hypothesis states that as anxiety increases there is a *general* decrease in the efficiency of behavior involving task-irrelevant (incidental) cues. For incidental learning, therefore, a uniform decrement in learning under heightened ego threat would be predicted regardless of the form of coping.

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The position of the present investigation holds that there is no unitary response to threat. The Easterbrook hypothesis seems applicable only to the cognitive behavior of repressers. Such subjects, coping with threat by avoidance, should narrow their attentive field; this would involve a decreased utilization or awareness of the incidental stimuli. Thus, it was predicted that only repressers would show a decrement in incidental learning under significant ego threat. Sensitizers, on the other hand, were expected to respond with vigilance and a general broadening of attention, which would involve greater utilization of the task-irrelevant, incidental stimuli. Therefore, it was predicted that with the introduction of significant ego threat, sensitizers would show an increase in incidental learning.

A dimension of significance to the cognitive consequences of differential defensive behavior concerns the relationship between affective connotation and learning. While the relevance of affective intensity and affective quality has remained inconclusive, there is some evidence that such factors are of influence (Weiner, 1966). And, as a general effect, for the three affective categories of stimuli used in the present study, it was predicted that incidental learning would be, in order of magnitude, positive, negative, neutral (see Amster, 1964).

The negative affect stimuli are expected to have the greatest "intrinsic" potential for anxiety arousal. For this affect category the incidental learning performance of the repressers and sensitizers should reflect maximal differential defensive behavior, that is, the most change under high ego threat. For the repressers, it was predicted that the expected general decrement in incidental learning under high ego threat would be greatest for the stimuli of negative affect value; for the sensitizers, the overall increment in learning, likewise, would be greatest for the negative affect stimuli.

## Method

### *Subjects*

The subjects were 209 female undergraduates from the State University of New York at Buffalo who were enrolled in introductory psychology courses. All female students in the introductory courses completed the revised version of the Byrne Repression–Sensitization (*R–S*) scale (Bryne et al., 1963). Based on the obtained distribution of *R–S* scores, the sensitizer group comprised those subjects with *R–S* scores at or above 80%, while the represser group comprised those subjects with scores at or below 20%.

A standard 2 × 3 × 2 factorial analysis-of-variance design with 12 subjects per cell was used. More than the required 144 subjects were tested, as it was expected that some subjects would fail to meet the criteria of the postexperimental inquiry (explained subsequently); 42 subjects were discarded for the previous reason. Any extra subjects in a given treatment condition were eliminated randomly to yield the 12 subjects required in each cell.

### *Stimulus Materials*

*The Byrne Repression–Sensitization scale.* The *R–S* scores for the repressers ranged from 3 to 22 ($M = 14.02$, $SD = 5.22$), with *R–S* scores for the sensitizers ranging from 57 to 106 ($M = 72.84$, $SD = 9.61$).

*The learning stimuli—intentional and incidental items.* The intentionally learned stimuli were 12 trigrams with percentage association values ranging from 50 to 70. All subjects were exposed to and attempted to learn the same list of 12 trigrams.

The incidental stimuli comprised three lists of 12 English words each, with a given subject exposed only to the words in one of the lists. Within each list the words were all of a similar affective value and were either all positive, all negative, or all neutral in affective connotation.

The selection procedure for the words was derived from previous research which had used the semantic differential procedure with a list of most frequently used English words (Heise, 1965). Samples of positive affect words (words with a rating close to 1), negative affect words (words with a rating close to 7), and neutral affect words (words with a rating close to 4) were chosen and semantic differential ratings were obtained in a pilot study. Based on the obtained ratings, the 12 most positive, the 12 most negative, and the 12 most neutral words were selected.

The words in the neutral affect list were: older, such, sigh, few, area, bring, box, very, wear, main, both, and soon (mean evaluative rating = 3.64). The words in the positive affect list were: love, peace, joy, best, happy, good, kiss, music, girl, nice, bed, and home (mean evaluative rating = 1.52). The words in the negative affect list were: kill, fail, war, bad, debt, dead, lose, hate, poor, worry, fear, and fight (mean evaluative rating = 5.91).

*Stimulus arrangement.* The stimuli were presented to a subject in pairs of one incidental and one intentional stimulus each, printed on the lefthand side of a blank card, with the incidental stimulus in the top position and the intentional stimulus below it. Over the eight experimental trials, each trial involving an exposure to the 12 incidental-intentional stimulus pairs, no specific incidental stimulus was ever paired more than once with a specific intentional stimulus, nor did any given intentional or incidental stimulus ever occupy the same serial position in the list more than once. Such procedures dissuaded subjects from attempting to use specific incidental stimuli as aids in learning the intentional stimuli and prevented learning by serial anticipation.

*Ego-threat manipulation.* Using differential verbal instructions, two levels of ego threat were employed. Instructions in the low ego-threat condition described the experiment as a normative learning study. The experimenter indicated that his interest was in obtaining "average" performance data for the general college population. For the high ego-threat condition, the instructions were intended to produce a high degree of induced anxiety. The experiment was described as investigating the relationship between learning and mental abilities. Learning of the trigrams was related to a subject's intelligence. Subjects also were informed that their performance would be compared with past measures of academic ability.

### *Procedure*

Subjects were tested in small groups. Instructions were played to the subjects on a Wollensak 1500 tape recorder.

The experiment was described as investigating how people learn new, unfamiliar words. To provide an explanation for the presence of the incidental English words, it was indicated that past research had found that the knowledge of one language can have a distracting influence on how one learns a new language. As the experimenter also was interested "in learning more about this effect," there would be a familiar English word present on each card along with the trigrams which the subject would be attempting to learn.

Each subject had a stack of cards containing eight sets of 12 cards each, corresponding to the eight experimental trials. Subjects turned the cards in time with a series of "beeps" coming from the tape recorder at 4-second intervals. With each "beep," the subject turned the card she was looking at and studied the trigram on the next card.

A 3-minute test of free recall, the test for incidental learning, was given 30 seconds after the end of the last learning trial. Subjects wrote as many of the English words as they could remember. Immediately after the recall test for the incidental stimuli, a similar 3-minute test of free recall was given for the intentionally learned trigrams. Subjects were instructed not to put down any additional English words they might recall during this period.

A subject's incidental learning score was the total number of English words correctly recalled. Likewise, a subject's intentional learning score was the number of trigrams correctly recalled.

### *Postexperimental Inquiry*

Following the recall tests, the postexperimental inquiry was conducted. The procedure was designed to be extensive and the areas of inquiry included: comprehension of instructions and procedures, general behavioral hypotheses and intentions, nature of response to the incidental English words, and expectation of retention test for the incidental stimuli.[3]

Inquiry booklets were independently evaluated by four advanced graduate students in psychology who were not aware either of the subject's treatment condition on her learning scores. A subject was not retained if her responses gave any evidence that she: misunderstood the instructions or procedures; for whatever reason, responded to the incidental stimuli with intentional learning operations (e.g., rehearsal, use as mnemonic devices in paired-associate behavior); expected a recall test for the English words; or attempted to learn the English words. Using these criteria, each judge rated the subject as belonging in one of three categories: (*a*) *include*—met all retention criteria, (*b*) *exclude*—failed one or more retention criteria, or (*c*) *uncertain*—unable to rate due to inconsistencies in the subject's responses.

For a subject's data to be used at least three of the four judges had to have rated that subject as "include." Of the 209 subjects tested, 167 subjects met the previous criterion. After extra subjects were eliminated randomly from the various treatment conditions, there was complete judges' agreement for 128 (89%) of the remaining 144 subjects.

## RESULTS

### *Incidental Learning of the English Words*

For each subject, incidental learning performance was the total number of the 12 English words recalled correctly. Bartlett's test for homogeneity of variance produced a nonsignificant fraction. A factorial analysis of variance for equal $N$'s was performed on the data.

The treatment-condition means and standard deviations are presented in Table 1. This analysis is referred to as the original analysis. As predicted, the main effect for the affect categories was significant ($F = 21.06$, $df = 2/132$, $p < .005$). There was the greatest amount of incidental learning for the positive affect words ($M = 5.97$), with the learning of negative affect words next ($M = 4.31$), and the least amount of learning for the neutral affect words ($M = 3.25$). The differences were in the expected directions and were significant at the .05 level or better. Both affective intensity and affective quality significantly influenced the amount of incidental material learned.

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[3] A copy of the questions may be obtained from the author.

TABLE 1

MEAN NUMBER OF INCIDENTAL STIMULI RECALLED IN ORIGINAL ANALYSIS

| Word affect category | Represser | | | | Sensitizer | | | |
|---|---|---|---|---|---|---|---|---|
| | Low ego threat | | High ego threat | | Low ego threat | | High ego threat | |
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Positive | 7.16 | 1.99 | 5.91 | 2.35 | 4.91 | 2.77 | 5.91 | 1.44 |
| Negative | 4.91 | 2.27 | 4.66 | 2.05 | 3.75 | 1.81 | 3.91 | 2.67 |
| Neutral | 2.83 | 1.46 | 2.91 | 1.67 | 2.58 | 1.44 | 4.66 | 2.34 |

The interaction between the represser-sensitizer variable and level of ego threat attained statistical significance ($F = 5.04$, $df = 1/132$, $p < .05$). Examining the performance of the repressers and sensitizers for the original analysis, as shown in Table 2, it is seen that when subjected to significant ego threat, the sensitizers showed an increment in incidental learning, while the repressers showed a decrement in learning. The changes were in the predicted directions.

It was predicted that the repressers, in addition to a general decrement in incidental learning under high ego threat, also would manifest an even greater decrement for the negative affect words; for the sensitizers, it additionally was predicted that the increment would be greatest for the negative affect words. The hypotheses were not supported in the original analysis.

The failure of the negative affect words to have the differential effects predicted may have resulted, in part, from the highly significant main effect found for the word affect variable. The very effect may not have allowed the other variables to manifest their expected interactional influences. Another analysis was performed attempting to attenuate the influence of the word affect variable.[4] The analysis will be referred to as the low affect-intensity analysis. Based on the semantic differential ratings and to attenuate the strength of the affect-intensity variable, the six words strongest in either positive or negative affect value were eliminated. These were: girl, bed, nice, home, kiss, music, and kill, fail, bad, war, debt, dead, respectively. For the neutral affect list, the six least neutral words were eliminated; these were: soon, both, main, wear, very, box.

The treatment-condition means and standard deviations for the low-affect-intensity analysis are presented in Table 3. The main effect of the word affect category was again significant ($F = 21.58$, $df = 2/132$, $p < .005$). The differences in learning for the positive affect words ($M = 3.20$), negative affect words ($M = 2.26$), and neutral affect words ($M = 1.60$) were as predicted and were again all statistically significant at the .05 level or better.

As indicated by Table 2 under high ego-threat conditions, as predicted, the repressers showed a decrement in amount of incidental learning, while the sensitizers showed an increase in learning ($F = 12.01$, $df = 2/132$, $p < .005$). While the interaction was also significant and in the predicted directions in the original analysis as well, in the present analysis the interaction attained a higher level of significance. This indication of greater change was consistent with the expectation that attenuation of the influence of the word affect variable should be reflected in an increase in the influences exerted by the other variables.

The data failed to support the hypotheses of differential defensive behavior for the negative affect words. While neither of the two analyses provided statistical support for the predictions, examination of Tables 1 and 3 suggested some evidence for differential effects (i.e., differences in learning between

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[4] The corollary, that if the word affect variable were strengthened the other variables would show even less of an influence, was also investigated by an analysis using only the six most affective words. As expected, only the word affect variable was significant.

TABLE 2

DEFENSIVE MODE AND RECALL UNDER LOW AND HIGH EGO THREAT

| Defensive mode | Original analysis | | | | Low affect-intensity analysis | | | |
|---|---|---|---|---|---|---|---|---|
| | Low ego threat | | High ego threat | | Low ego threat | | High ego threat | |
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Represser | 4.97 | 2.64 | 4.50 | 2.34 | 2.83 | 1.43 | 2.11 | 1.27 |
| Sensitizer | 3.75 | 2.31 | 4.83 | 2.31 | 1.91 | 1.42 | 2.58 | 1.40 |

low and high ego-threat levels). In the original analysis, as indicated in Table 1, for the sensitizers the increase in learning with high ego threat appeared largest for the neutral affect words and least for the negative affect words. For the repressers the only decrement of note appeared for the positive affect words. Considering the low-affect-intensity analysis scores, shown in Table 3, the sensitizers still appeared to show the most increment in learning under high ego threat for the neutral affect words and the least difference for the negative affect words. The order was reversed for the repressers with the greatest apparent decrement in learning for the negative affect words. The repressers thus seemed to show performance that was at least consistent with the experimental prediction—the greatest decrease in incidental learning for the most anxiety-arousing words.

### *Intentional Learning of the Trigrams*

An analysis of variance of the subject's learning scores for the trigrams did not provide any evidence of either main or interaction effects. None of the experimental variables had any influence on intentional learning. An analysis of covariance was done with trigram scores as the covariate for both the original and low-affect-intensity analyses. The results of both yielded adjusted treatment means almost identical to the unadjusted means. It is thus most unlikely that differences in learning ability were involved in the obtained differences in incidental learning.

### *Intellectual Ability*

While not obtained directly from subjects during the experiment, a measure of intellectual ability became available for all subjects. Analysis of variance of these scores did not disclose any significant differences between the conditions. In an analysis of covariance for the original and low-affect-intensity analyses, adjusted treatment means were almost identical to the unadjusted means. Differences in intellectual ability were not likely to have entered into the obtained differences in incidental learning performance.

### *Postexperimental Inquiry*

Validity of the rating procedure would require evidence that the subjects in the three different rating categories did show differences in incidental learning. For subjects in the "exclude" category, recall scores should be greater than for subjects in the "include" category. And, since subjects in the "uncer-

TABLE 3

MEAN NUMBER OF INCIDENTAL STIMULI RECALLED IN LOW-AFFECT-INTENSITY ANALYSIS

| Word affect category | Represser | | | | Sensitizer | | | |
|---|---|---|---|---|---|---|---|---|
| | Low ego threat | | High ego threat | | Low ego threat | | High ego threat | |
| | *M* | *SD* | *M* | *SD* | *M* | *SD* | *M* | *SD* |
| Positive | 3.66 | 1.07 | 3.00 | 1.12 | 2.75 | 1.54 | 3.41 | 0.99 |
| Negative | 3.16 | 1.33 | 1.91 | 0.99 | 2.00 | 1.20 | 2.00 | 1.34 |
| Neutral | 1.66 | 0.98 | 1.41 | 1.24 | 1.00 | 0.85 | 2.33 | 1.49 |

tain" category were judged to have responded in an inconsistent manner, the recall scores for such subjects might well fall between those of the other two categories.

The mean incidental learning scores were consistent with the previous reasoning. For the "include," "uncertain," and "exclude" categories, the learning means were 4.51, 6.87, and 8.32, respectively.

An extensive postexperimental inquiry procedure was employed with approximately 20% of the subjects eliminated for failing to meet the retention criteria. That 20% of subjects were eliminated via the inquiry procedure indicates the need to include such assessment procedures in all incidental learning research.

## DISCUSSION

### *Defensive Mode and Ego Threat*

The findings supported the assumption that the influence upon learning of an individual's mode of coping with ego threat could be specified on the basis of a prior knowledge of this characteristic defensive style. Sensitizers and repressers were differentially influenced in their incidental learning by the introduction of ego threat. Repressers responded with a decrease in incidental learning, sensitizers with an increase. The differences in manner of response to ego threat were consistent with the view that associated with different modes of defense are equally distinct cognitive orientations in responding to environmental stimulation. And such patterns are in accord with more clinically based views as to how such defenses operate: "awareness is narrowed by repression and broadened by isolation [i.e., sensitization—Luborsky et al., 1965, p. 279]."

An important difference is to be noted between the present research and some earlier studies on defensive behavior. In the present investigation, the two defensive groups each showed a clearly different cognitive response to threat. In the Eriksen studies (1966) it was generally found that performance of the represser groups showed evidence of cognitive avoidance while the sensitizer groups showed little or none of such behavior. While Eriksen's studies provided evidence for a represser group which when threatened resorted to avoidant-type defense, the interpretation of the other group, which did not show the defensive behavior, as sensitizers is questionable. The difficulty may lie in Eriksen's use of completed/incompleted task-recall scores, interpreted as reflecting differential ego-defensive behavior, to obtain the two defensive groups. Achievement and affiliative needs have been shown to exert a significant influence on such recall (Atkinson, 1947; Atkinson & Raphelson, 1956).

With respect to the effect of anxiety arousal on performance, the Easterbrook cue-utilization hypothesis held that anxiety arousal disorganizes the utilization of cues by a general narrowing of the range or field of attention. The present findings were at variance with the hypothesis. The presence of a facilitative as well as an inhibitory effect on incidental learning, as a function of an individual's characteristic method of coping with such anxiety, effectively argues against Easterbrook's contention. For the sensitizer, anxiety arousal enhanced incidental learning, while for the represser, it proved to have a negative influence. In Easterbrook's terms, under conditions of anxiety evocation, the sensitizers gave evidence of greater utilization of environmental stimuli while the repressers manifested a lowered degree of cue utilization. The Easterbrook hypothesis would seem to reflect a drive-theory interpretation which omits consideration of the possible role of personality attributes. The differential performances found associated with avoidant versus approaching modes of responding to anxiety arousal make clear the limitations in such an approach. As Lazarus has noted, "Prediction of performance changes under stress requires specification of those coping processes which intervene between threat and adaptive functioning [Lazarus, 1966, p. 364]."

### *Defensive Mode and Affect Value*

While past research on affective connotation was unclear, both the affective intensity and quality of the English words were expected to be related to amount of incidental learning. Such hypotheses were supported with learning greatest for the positive affect words and least for the neutral affect words. Unlike most previous studies (except for

Amster, 1964), the present research used an incidental learning task. It appears that in reducing the degree of voluntary control which subjects exert on the stimuli (e.g., rehearsal, mnemonic devices), the possibility of such activities obscuring the influence of affective value was markedly reduced.

An interaction of the English words with the represser-sensitizer and ego-threat variables also was predicted; under significant threat the negative affect words would call forth the most defensive behavior for the repressers and sensitizers. The expectations were not substantiated. But the low-affect-intensity analysis provided some evidence that the main effect for words had obscured, to a degree, the effect of the other variables. And an examination of the treatment conditions appeared to provide suggestive indications of interaction, but not necessarily in the manner expected. While the original analysis was somewhat inconclusive, for the repressers the low-affect-intensity analysis did suggest, consistent, at least, with expectation, the greatest decrement for the negative affect words. But the defensive behavior for the sensitizers was just opposite to prediction. Instead of the greatest increment, in both analyses the sensitizers appeared to show the smallest increment for such words.

A question arises as to the sensitizer's apparent failure to show evidence of sensitization-type defenses for the condition in which it was most expected. Some modification of the concept of sensitization may be in order, namely the inclusion of the defense of projection. With the intensification of threat, the defensive behaviors operating may have shifted from those sensitizing defenses whose primary behavioral manifestations reflected greater accuracy to those defenses primarily associated with diminished efficiency, if not actual distortion of the stimuli, and thus poorer accuracy. It is possible that the various defense mechanisms associated with sensitization need not necessarily be manifested only in heightened efficiency. The relevance of the sensitizing defense of projection becomes clear in examining how the person using projective defenses views his environment. Shapiro (1965) has noted that sensitivity and alertness are cardinal traits. "Their attention is not only unusually acute and intense but unusually active. It is . . . an actively scanning, searching attention [Shapiro, 1965, p. 151]." The previous description parallels the earlier description of the "vigilant" sensitizer who also employs scanning behavior involving pronounced "looking-around" response patterns. The individual apt to be aware of and alert to threat may also be prone to exaggerate that threat under certain conditions.

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# HEART-RATE COMPONENTS OF CONDITIONED VICARIOUS AUTONOMIC RESPONSES[1]

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The present study examined heart-rate and skin-resistance responses within a paradigm for the classical conditioning of vicariously instigated autonomic responses using electric shock to a model as the unconditioned stimulus (UCS). Fifty-six undergraduate observers were assigned to one of four groups in which subjects either (*a*) anticipated going through the model's task, (*b*) were assured that such would not be the case, (*c*) simply observed the model move, rather than be shocked, or (*d*) did not see the model move or be shocked. Results indicated that both galvanic skin responses and heart rate could be conditioned. Observing the model being shocked or simply moving produced heart-rate deceleration of varying degrees of magnitude. The implications of the finding for models of vicarious learning and emotional sensitivity to others are discussed.

The classical conditioning of electrodermal activity instigated by observing others in distress has been examined recently by several investigators (Bandura & Rosenthal, 1966; Berger, 1962; Craig & Weinstein, 1965; Haner & Whitney, 1960). Such studies have been used to explain many social learning phenomena by suggesting that the vicarious experience can substitute for direct experience in the same situation. However, recent studies suggested that the pattern of autonomic arousal induced vicariously is not comparable to arousal instigated by direct contact with noxious stimulation. Craig (1968) noted that whereas both direct and vicarious contact with the cold pressor test produced increases in skin conductance, only the direct experience was associated with cardiac acceleration, while the vicarious experience tended to produce cardiac deceleration. Further, Craig and Wood (1969) observed similar physiological differentiation, but noted that those who experienced more distress in the task situation did not manifest deceleration to the vicarious exposure. Lacey and his associates' (Lacey, 1967; Lacey, Kagan, Lacey, & Moss, 1963) observations that cardiac deceleration accompanies and perhaps facilitates ease of "environmental intake" would appear to have direct implications for the findings, since efficient learning of the nature of the situation would be facilitated by cardiac deceleration. This would be consistent with reports indicating that observational learning can be more efficient than learning through direct reinforcement (Flanders, 1968; Kanfer & Marston, 1963).

Cardiac rates appear to reflect attentional processes, and vicarious learning appears to be the result of a complex interaction between affective and cognitive states (Bandura, 1965; Berger, 1968). The present study examined both cardiac and galvanic skin-resistance responses (GSRs) within a paradigm for classical conditioning of vicariously instigated autonomic responses using a model's discomfort, as a consequence of electric shock, as the unconditioned stimulus (UCS). Other investigators have restricted their analyses to GSR changes. It was hypothesized that (*a*) observing others receiving electric shock is associated with heart-rate deceleration and increased numbers of GSRs; and (*b*) the cardiac responses and skin-resistance changes can be conditioned to stimuli signaling the imminent onset of shock.

Since Berger (1962) has suggested that arousal produced by situations in which the observer anticipates having to undergo the

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noxious experience is only "pseudovicarious" arousal, a group which was advised to anticipate going through the model's task was compared with another group who was assured that such would not be the case. It was predicted that the instructions to the former group would produce more arousal as reflected by increased GSR activity and higher heart rates. An additional group was included to control for the possibility that simple movement rather than aversively induced movement would instigate autonomic changes comparable to that induced by the latter experience.

## METHOD

### *Subjects*

The subjects were 56 volunteer students from the University of British Columbia. There were 28 males and 28 females, and the number of males and females in each of the four groups was balanced. The mean age of the subjects was 22 years, and the range was from 18 years to 38 years. Assignment to each of the four groups was random. A paid male confederate performer (P) was used in all groups.

### *Apparatus*

A Grass Model 7 polygraph was used to record skin resistances and heart rates. Beckman biopotential electrodes were placed on the palmar surface of the first and fourth fingers (third phalanx of the fingers) for GSR recording and on the right wrist and midlaterally on the calf of the right leg for heart rates.

A Gerbrand's timer was used to trigger a Sawyer projector at the same time as it triggered the stimulus marker on the polygraph. Seven seconds after stimulus onset the timer delivered a click signal to the P.

Stimuli were 10 colored slides of innocuous objects, such as a ball, ruler, and so forth. Each stimulus was presented for 12 seconds. There were five trials, and the order of presentation was randomized across trials. The interstimulus interval ranged from 19.50 to 34.25 seconds, with an average interval of 23.60 seconds. Five of the stimuli (randomly chosen) were classified as experimental stimuli, accompanied either by faked shock to P, movement of P's arm, or nothing in the control group. The other five were classified as control stimuli.

### *Procedure*

Subjects were run in pairs; however, only one was a naive subject and the other was the confederate. Upon arrival they were "randomly" assigned to the roles of observer (*O*) and P. They were told that the experimenters were interested in physiological responses during learning. The *O* was told that his partner would be asked to perform a task, but that all he, *O*, would be required to do was to act as a control and observe P and the pictures he was watching. In the case of the group of *O*s expecting to be shocked, permission to use mild electric shock was obtained, with the explanation that the experimenters were interested in the effects of mild shock on the learning process. One subject refused and was replaced; during one experimental procedure it became evident that another subject was deliberately trying to confound the results, and he, too, was replaced. The P was told, in all cases, that he was to learn the pictures silently and that he would be asked to remember them on a questionnaire later. His permission to be shocked was always obtained.

Four groups were used and given instructions in addition to those previously mentioned:

*Control group.* The *O*s were told that they would not have to do the task later.

*Move group.* The P was asked to pick any five pictures and jerk his arm when he heard a click while the picture was on. He was also told to make sure he jerked his arm to the same five each time they were shown. The arm jerk was demonstrated. The rationale given for the instruction was that movement was assumed to affect the learning process. The P had previously been trained not to behave any differently while in this condition than in the following two shock conditions.

*Will group.* The *O* was told that he would undergo the identical task as his partner when his partner finished. The P was told that he would be shocked while watching some of the pictures and that the same ones would be acompanied by shock each time through the series.

*Won't group.* The *O*s were reassured that they would not receive shock at any time during the experiment, nor would they have to perform the task afterwards.

After the instructions, the electrodes were connected and subjects were asked to sit comfortably and as quietly as possible. An adaptation period of approximately 10 minutes followed.

The room lighting was dimmed to .5 footlambert during all experimental recording phases. The lighting was then increased to a normal visual level, and a questionnaire was administered to assess the degree of learning of experimental and control stimuli, to get *O*'s likability rating of P, and to assess *O*'s subjective emotional feelings during shock to P.

### *Physiological Measures*

The following indexes were obtained for each subject on each exposure of a slide:

*Heart rate.* The mean of all beats occurring wholly or partly within the following eight 2-second intervals was calculated: two intervals prior to stimulus onset; two intervals immediately following stimulus onset; the following two intervals preceeding and overlapping by 1 second the moment when shock was ostensibly administered or the moment when P jerked his hand (this allowed for P's reaction time and the latency of heart-rate changes); and, finally, the last two 2-second intervals occurring after the UCS.

*Skin resistance.* GSR responses were defined as accelerating changes in the direction of decreasing skin resistance of 1,000 ohms or greater. The scoring procedure has been shown to be reliable by several investigators (Bandura & Rosenthal, 1966; Craig & Weinstein, 1965). Following Stewart, Stern, Winokur, and Fredman's (1961) use of reduced latency criteria for GSR conditioning, those GSR responses occurring after 4 seconds subsequent to stimulus onset, through the following 4 seconds overlapping UCS onset by 1 second, were scored as conditioned anticipatory responses. Those GSR responses occurring during the following 4 seconds were scored as vicariously instigated responses.

Scores for both heart-rate and GSR measures consisted of the sum of the scores to either the five experimental or five control stimuli administered during that trial.

## RESULTS

Analyses of variance on the dependent variables were performed over the following variables: experimental group, sex, stimuli, and trials. Dependent measures were: (*a*) mean heart rate in each of the eight measurement intervals and (*b*) frequency of GSR responses during the anticipatory and vicarious periods. Questionnaire answers were examined using the previous independent variables where appropriate.

In view of the observation (Haggard, 1945) that basal tension levels may be related to the magnitude of GSR reactivity, basal readings taken 1 minute prior to the end of the adaptation period and prior to the experimental operations were examined for group differences. Analyses of variance on both heart rate and skin conductance over the variables of groups and sex did not disclose reliable differences in either case ($p > .10$).

### *Heart Rate*

Heart rates are plotted in Figure 1 for both the experimental and control stimuli for all groups over the eight measurement periods. All trials are averaged. Inspection of Figure 1 reveals:

1. The four groups ordered themselves during all measurement periods prior to the vicarious arousal period, with the group anticipating having to proceed with the shocked P's task (will group) manifesting the highest heart rate; the group observing the shocked P but not anticipating having to proceed with the task (won't group) manifested the lowest heart-rate; and the group observing the unshocked but moving P (move group) manifested a higher heart rate than the won't group, but less than the group observing the unshocked or unmoving P (control group).

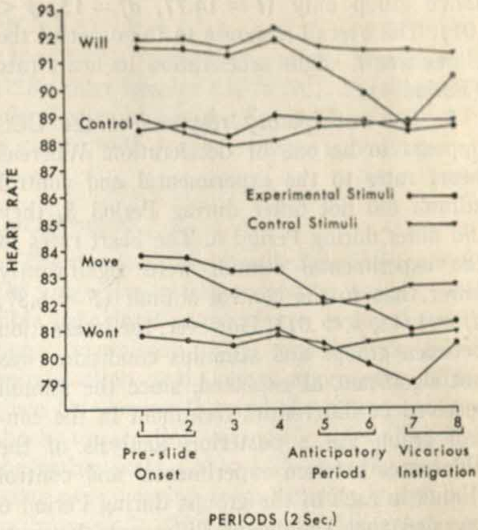

FIG. 1. Mean heart rates for groups over measurement periods.

The main effect for group differences was significant in each of the eight measurement periods. Composite scores consisting of the mean heart rate for each subject over the five periods prior to the onset of differential responses to the experimental and control stimuli were examined with a one-way analysis of variance over groups ($F = 7.02$, $df = 3/36$, $p < .01$).

A Duncan range test indicated that differences between all pairs of groups were significant other than those between groups "move" and "won't," and "control" and "will" ($p < .05$). Thus, decelerative effects in the two former groups were significantly below the control measure, and the threat of shock (will group) appears to result in a higher tonic level of heart rate.

2. Experimental and control stimuli did not appreciably differ from one another during the first five periods, although during Period 4 the difference between the response to the experimental ($M = 83.25$) and control ($M = 81.93$) stimuli was significant for the

move group only ($t = 14.77$, $df = 13$, $p < .01$). The overall response to the onset of the slides was a slight acceleration in heart rate (Period 4).

3. The anticipatory response to the UCS appears to be one of deceleration. Whereas heart rates to the experimental and control stimuli did not differ during Period 5, they did differ during Period 6. The heart rates to the experimental stimuli were significantly lower than to the control stimuli ($F = 6.87$, $df = 1/48$, $p < .01$). However, the interaction between groups and stimulus conditions was not significant as expected, since the stimuli received no differential treatment in the control group. An a posteriori analysis of the differences between experimental and control stimuli in each of the groups during Period 6 revealed that significant differences between experimental and control stimuli appeared only in the group where the *O*s viewed P being shocked and anticipated shock themselves (will group—$F = 8.26$, $df = 1/16$, $p < .05$).

4. Observing P ostensibly being shocked produced decelerative heart-rate changes. Such was also apparently the case for subjects observing P simply move, although the latter difference also could be a consequence of the apparent increase in heart rate to the control stimuli at that time. The analysis of variance disclosed a significant Group × Stimulus Conditions interaction during Period 7 ($F = 2.73$, $df = 3/48$, $p < .05$). Heart rates to the experimental and control stimuli significantly differed for each individual group ($F = 21.98$, 9.44, and 7.39, $df = 1/16$, $p < .05$ for will, won't, and move groups, respectively) other than the control group.

A Duncan range test comparison of the magnitude of the difference between experimental and control stimuli across the three groups where this differed indicated that the group anticipating having to undergo the experience (will group) differed more than the others ($p < .05$), but that the others did not differ from each other.

The tendency for a differential response to experimental and control stimuli disappears during Period 8, although the stimuli still differ significantly in their impact ($F = 5.19$, $df = 1/48$, $p < .05$). A significant interaction of the response to stimuli and trials ($F = 3.26$, $df = 4/192$, $p < .05$) indicated that the differential response to stimuli disappeared after Trials 1 and 2.

*Skin-Resistance Changes*

Analyses of the changes in skin resistance were expected to confirm expectations that the frequencies of vicariously instigated and conditioned GSR responses would be a function of the various treatment conditions.

*Vicarious instigation.* Significant effects were associated with the experimental instructions, stimulus differences, and subject sex as indicated in Table 1, summarizing the analysis of variance on the data. Effects associated with the experimental instructions and stimulus differences were best represented by the significant interaction between the variables. The two groups observing the shocked P manifested the highest numbers of GSRs; the group observing the moving P, the next highest; and the group observing the control P, the fewest. A Duncan range test indicated that the difference between the will and won't group means was not significant, but both groups manifested more GSRs than the move group ($p$'s $< .01$). The move group, in turn, had more GSRs than the control group ($p < .01$). In all but the control group the experimental stimuli instigated significantly

TABLE 1

ANALYSES OF VARIANCE ON VICARIOUSLY INSTIGATED GSRs

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Sex (A) | 1 | 78.00 | 15.53** |
| Group (B) | 3 | 30.03 | 5.98** |
| AB | 3 | .57 | <1 |
| Error between | 48 | 5.02 | |
| Stimuli (C) | 1 | 492.19 | 264.69** |
| AC | 1 | 2.72 | 1.46 |
| BC | 3 | 52.90 | 28.45** |
| ABC | 3 | 5.21 | 2.80* |
| Error for stimuli | 48 | 1.86 | |
| Trials (D) | 4 | 23.31 | 24.38** |
| AD | 4 | 1.20 | 1.25 |
| BD | 12 | 2.52 | 2.63** |
| ABD | 12 | 1.18 | 1.23 |
| Error for trials | 192 | .95 | |
| CD | 4 | 12.90 | 17.27** |
| ACD | 4 | .18 | <1 |
| BCD | 12 | 1.01 | 1.36 |
| ABCD | 12 | 1.17 | 1.57 |
| Error residual | 192 | .74 | |

* $p < .05$.
** $p < .01$.

more GSRs ($F$'s $= 55.31$, $169.40$, and $124.46$, $df = 1/48$, $p < .01$) than the control stimuli. The latter observation must be qualified in light of the significant interaction of group and stimulus conditions with sex differences. Females were not as reactive to the experimental stimuli when assigned to the move and will groups. However, females still had more vicarious GSRs to the experimental stimuli than to the control stimuli ($F = 12.91$, $df = 1/48$, $p < .01$). Over all conditions and groups, the females were not as reactive as the males (mean number of GSRs for males $= 2.26$; for females $= 1.51$). Habituation effects can be observed in the significant decrease in GSRs over trials ($p < .001$). The interaction of stimuli with trials indicated that the frequency of GSRs to the stimuli decreases over time, with the frequency to the experimental stimuli declining more rapidly, and the interaction of group effects with trials indicated that the decline in the frequency of GSRs was not as rapid in the control and will groups.

*Conditioned vicarious arousal.* Analyses on the frequencies of conditioned vicarious GSRs were restricted to the final four trials, since anticipatory responses could not be expected to occur until after the first trial. The mean frequencies of anticipatory GSRs are provided for groups in Table 2. Analysis of variance indicated a significant Group × Stimuli interaction ($F = 2.71$, $df = 3/48$, $p < .05$). Analyses of simple effects revealed that only in the won't group was there a differential response to the experimental and control stimuli ($F = 11.37$, $df = 1/48$, $p < .01$). Contrary to expectations, relatively few GSR's were observed in the will group, either to the experimental or the control stimuli. Significant sex differences were observed ($F = 8.32$, $df = 1/48$, $p < .01$), with males ($M = 1.30$) providing more conditioned GSRs than females ($M = .70$). Finally, trial effects were observed ($F = 2.07$, $df = 3/144$, $p < .10$), with fewer responses appearing over time.

TABLE 2

FREQUENCIES OF ANTICIPATORY GSRS TO EXPERIMENTAL AND CONTROL STIMULI OVER GROUPS

| Stimuli | Groups | | | |
|---|---|---|---|---|
| | Control | Moves | Will | Won't |
| Experimental | 1.32 | .88 | .88 | 1.38 |
| Control | 1.43 | .75 | .68 | .73 |

### *Questionnaire*

The questionnaire was designed to evaluate *Os*' subjective reactions and to suggest possible behavioral consequences of the observational experience. The confederate's faked pain reaction to the shock appeared credible, since *Os* viewing it rated the severity of the shocks toward the "extremely painful" end of the rating scale ($M = 1.77$ on a 9-point bipolar scale varying from "extremely painful" to "pleasant"). There were neither group nor sex differences on the scale. Further, *Os* tended to rate their own feelings toward the "extremely painful" end of the scale while observing P being shocked. Females rated their reactions as being more painful ($M = 3.36$ on a 9-point rating scale varying from "extremely painful" to "pleasant") than did the males ($M = 4.94$—$F = 8.19$, $df = 1/24$, $p < .01$).

When *Os* rated how they felt towards P on a 9-point scale varying from "liked" to "disliked," they tended to rate their response toward the positive end, with the females expressing more affection ($M = 2.71$) than males ($M = 3.57$; $F = 4.05$, $df = 1/48$, $p < .05$). Group differences as a consequence of the experimental operations did not appear.

Since attentional processes were hypothetically related to the physiological processes under scrutiny, the possibility that group differences in the number of stimuli remembered was examined. The *Os* tended to remember most of the items ($M = 8.9$) and all the experimental stimuli ($M = 4.1$). Group differences were not significant.

## DISCUSSION

The present study is consistent with earlier observations (Craig, 1968; Craig & Wood, see Footnote 3) suggesting that observing another in distress can be accompanied by heart-rate deceleration. In particular, such seems to be the case in experimental situations selected by others (Bandura & Rosenthal, 1966; Berger, 1962) to demonstrate that vicarious arousal can be classically conditioned. In the earlier studies there was concordance between the direction of changes in skin resistance and what one observes when subjects experience direct noxious stimulation. However, such apparently would not be the case when heart-rate indexes of arousal are used. When strong, stressful stimuli are actually presented to subjects, heart rate accelerates (e.g., Hastings & Obrist, 1967; Lacey et al., 1963; Lazarus, Speisman, Mordkoff, & Davison, 1962), whereas heart-rate deceleration was observed in the present instance where direct aversive stimulation was not experienced. Evidence indicating that the *O*'s experience was somewhat aversive was found on the postexperimental questionnaire.

Since there is apparently physiological differentiation of the vicarious response and the usual response to noxious stimulation, and subjective experiences of emotions are felt to be caused at least in part by the pattern of autonomic arousal, one could question whether the *O*s experienced an empathetic response similar or identical to that of the P, assuming that he was, in fact, being shocked. This is of importance, since, as Berger (1968) observed, theoretical explanations of vicarious learning processes hinge upon whether one's concept of vicarious experience assumes that the *O* shares the experience of the other or that it refers in a descriptive manner to matched similarities in the *O*'s and the P's behavior.

The evidence for physiological differentiation of vicarious and direct noxious experiences and the finding that both skin resistance and heart rate can be conditioned to vicarious arousal in others were consistent with classical conditioning explanations of the emergence of the capacity to recognize emotions in others. The cues associated with distress in the model came to have the capacity to evoke arousal in the *O*s. This deviates from McDougal's (1908) concept of "primitive passive sympathy," since, in his theory, the expression of emotion in one individual is felt to be the innate adequate stimulus for the same emotion in the *O*.

If vicarious experience originates in shared pleasant and unpleasant experiences with others, Berger's differentiation of vicarious and pseudovicarious arousal would appear to be unnecessary. It is worth noting that the "pseudovicarious" arousal group did differ quantitatively from the "vicarious" arousal group, but that they could not be differentiated on the basis of the direction of physiological changes.

The findings suggest that the magnitude of autonomic change in *O*s is not necessarily positively correlated with the intensity of the subjective experience of the *O*. Thus, whereas GSR changes consistently were observed more frequently among the males, the females reported on a questionnaire that they were more uncomfortable while watching the P in distress, and they also felt that the P was experiencing more pain than did the males. That it was, in fact, the pain putatively accompanying the shock that produced the autonomic changes in the *O*s can be argued, since the groups observing the P ostensibly receiving shock manifested larger changes in heart rates and GSRs than did those who simply observed the P moving.

A great deal of research and behavior theory on avoidance behavior is based on experimental paradigms in which subjects are the direct recipients of punishment or noxious stimulation. Yet, there is general acknowledgment in the literature today indicating that direct learning can be inefficient, and observational learning of paramount importance, in describing the developmental years of children. In view of the possibility that direct and vicarious arousal can be psychophysiologically differentiated in some situations, further studies using observational learning paradigms appear to be necessary.

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# EFFECTS OF ANXIETY AND SOCIALLY MEDIATED ANXIETY REDUCTION ON PAIRED-ASSOCIATE LEARNING[1]

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Investigations of anxiety reduction as a function of affiliation have tended to utilize self-report or physiological data as the dependent measure. The present study was undertaken to demonstrate the use of a third, behavioral, measure in such investigations, namely, verbal learning. Behavioral, self-report, and physiological measures were taken from 60 firstborn female subjects. The data from all three sources suggest that it is possible to select behavioral measures, such as paired-associate learning, that reflect changes in anxiety levels that are usually inferred from self-report or physiological data. However, it is suggested that, given present imperfect knowledge of these three measures, the use of a *combination* of them, as previously recommended by Martin and by Walters and Parke, provides a more comprehensive measurement of anxiety and anxiety reduction than the single index (self-report or physiological measure) that is customarily employed in social-psychological experiments.

There are two traditions of research in the psychological literature, seemingly unrelated, which concern the influence of anxiety on human performance. In the first tradition are studies, found mainly in the literature on learning theory, which indicate that individual differences in anxiety (as defined by the Taylor MA scale) facilitate performance on a simple paired-associate task, but impede performance on a complex one (Spence, Farber, & McFann, 1956; Spence, Taylor, & Ketchel, 1956; Taylor & Chapman, 1956). Other studies in this area have demonstrated that similar effects on performance (to those found with the *MA* scale) can be obtained through the use of experimentally induced anxiety, such as electric shock (Chiles, 1958; Lee, 1961) and psychological stress (Gordon & Berlyne, 1954; Lucas, 1952).

Investigations in the second tradition are concerned with the constructs anxiety *and* anxiety reduction, and have been pursued primarily by social psychologists. These studies have focused on social-emotional behavior manifested in response to anxiety induction, notably affiliative behavior (Gerard & Rabbie, 1961; Sarnoff & Zimbardo, 1961; Schachter, 1959) and on anxiety reduction as a consequence of affiliation (Kissel, 1965; Ring, Lipinski, & Braginsky, 1965; Wrightsman, 1960). For example, in a series of studies, Schachter (1959) has found a positive relationship between manipulated anxiety and affiliative responses; this relationship, however, held for firstborn and only child subjects but not for later borns, and was most clearly demonstrated with female subjects. Schachter has further hypothesized that under anxiety-producing conditions, affiliation should lead to anxiety reduction. Substantial support for this proposition, in the case of firstborns, was given by Wrightsman (1960), but it has received only qualified support from later studies (e.g., Kissel, 1965; Ring et al., 1965). In all of the above-mentioned studies self-report or physiological measures have typically been employed.

The purpose of the present research was the attempt to provide a behavioral measure of anxiety reduction to supplement the self-report and physiological measures generally employed in social-psychological studies of anxiety and anxiety reduction. This research, in fact, employs all three indexes of anxiety as dependent measures, a procedure advocated by Martin (1961) and by Walters and Parke

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(1964) as an aid in further clarification of the area of anxiety.

To provide a behavioral measure, a paired-associate verbal learning task was employed in both the anxiety-induction and anxiety-reduction phases of the experiment. In line with Spence's drive theory, it was expected that the performance on a simple task of experimental subjects who were exposed to the anxiety induction would be superior to that of subjects who were not exposed to this manipulation. It was further expected, consistent with the same drive theory, that anxiety reduction following affiliation (Wrightsman, 1960) would be manifested, behaviorally, in improved learning of complex verbal material. These expectations, and the rationale for them, are summarized in the following hypotheses:

*Hypothesis 1:* Prior to other experimental manipulations, firstborn female subjects who anticipate and experience shock during the learning of a *simple* paired-associate verbal learning task are more anxious than firstborn subjects who do not anticipate shock. Consequently, the shock subjects perform better than the no-shock subjects on such a task. It is hypothesized, in other words, that the aforementioned findings of Spence and his associates are replicated.

*Hypothesis 2:* Firstborn female subjects who receive shock and then affiliate experience greater reduction in anxiety than shocked firstborn subjects who are not allowed to affiliate following anxiety arousal. The anxiety level of the shocked subjects who do not affiliate is greater than that of firstborn female subjects who do not experience shock. Consequently, following affiliation, the performance of the shocked, nonaffiliated group on a *simple* paired-associate learning task is superior to the performance of the shocked subjects who affiliate and the no-shock groups. However, on a *complex* paired-associate task the performance of the shocked, nonaffiliated group is inferior to the other groups.

In addition to the behavioral measure, self-reports of anxiety and a physiological measure (heart rate) were obtained during the experiment to assess the effectiveness of the anxiety and anxiety-reduction manipulations (see *Measures*).

## METHOD

### *Subjects*

The subjects were 60 firstborn female undergraduates (8 were only children) enrolled in the introductory psychology course at the University of Waterloo. The majority of them participated for course credit. (Sixty-two subjects were recruited but 1 refused to participate and 1 had to be cancelled because of equipment failure.) The 60 subjects for whom data are reported were randomly assigned to four experimental treatments: shock-affiliation, shock-nonaffiliation, no-shock–affiliation, and no-shock–nonaffiliation, defined by the experimental manipulations that are described under *Procedure.*

### *Confederates*

Fifteen sets of first-year female students (three students per set), enrolled at a neighboring university, were employed—one set for each of the affiliation-condition sessions. All confederates served in both shock-affiliation and no-shock–affiliation conditions.

### *Experimental Arrangements*

Two rooms, with a one-way vision screen between them, were used in this experiment. The experimental room was carpeted and contained upholstered chairs, end tables, and smoking stands. In one corner of this room a cubicle was installed and equipped for presentation of verbal material and for administration of electric shock. The verbal material was presented on a Stowe 459B memory drum. The observation room contained a Beckman type RS dynograph with a heart-rate coupler, as well as the reception components of a Beckman single-channel telemetering system. The transmission components of the system were placed on the subject. The telemetering system permitted continuous recording of the subject's heart rate.

*Shock apparatus.* Subjects in the shock condition were given four electric shocks by a Lafayette Instrument Co. 1½-volt DC inductorium (Number 2380), attached to a 1½-volt Burgess Number 6 ignition dry-cell battery. Shock settings were so placed as to deliver a minimum of 5 milliamperes and a maximum of 13 milliamperes peak-to-peak current at 83 cycles per second and a pulse width of .2 millisecond. The minimum and maximum settings were determined beforehand from the reactions of nine female volunteers; a setting midway between these two extremes was chosen as the locus of the second shock. In practice, the first shock, preceded by the shock instructions, was delivered at the lowest setting; the second shock was given at the intermediate setting; and the third and fourth shocks were given at the highest setting.

*Verbal material.* The initial paired-associate list was made up of 12 high-association pairs of items (these were the simple pairs); the final paired-associate list was composed of 6 pairs left unchanged from the initial list and 6 pairs which were made by re-pairing the remaining stimuli and responses from the initial list (and considered as complex, low-association pairs).[3] The anticipation method was used in presenting the items, with a 2-second, 2 second rate of presentation and a 12-second intertrial interval.

### *Procedure*

On arrival at the experimental room, the subject was introduced to the experimenter and to a female assistant, and was told:

> We have reason to believe that there are some factors which influence a person's verbal learning ability; but so far we have no definite information on whether these factors facilitate or impede this learning ability. We are doing some research at the present on this problem, and have brought a few of you girls here today to assist us in this task. You will work with me in this room; the other girls are assigned to other rooms where they will be doing exactly the same task as you. Now, what we are going to do is this. . . .

The experimenter went on to explain the operation of the memory drum and the paired-associate learning task, and added that in the later learning sessions of the experiment some of the pairs of words would be the same as in the first learning session while the other pairs would be switched around. The subject was further informed that while she was learning physiological (heart rate) measures would be taken. Following the subject's acquiescence to measurement of her heart rate, the experimenter went to the other room while the female assistant attached the heart-rate electrodes and telemeter transmitter to her. Upon receiving a signal that the electrodes were plugged into the transmitter, the experimenter returned to the experimental room, which the female assistant had left a moment before.

The experimenter engaged the subject in "small talk" to secure a base line for the physiological measures as well as to attempt to allay any initial anxiety the subject might have been experiencing, after which each subject in the shock condition was told: "One of the things we spoke about earlier, which could affect learning, is arousal through electric shock. . . . The experimenter went on to indicate that while she was learning, a few fairly strong electric shocks would be given from time to time, which would be painful but temporary in their effects, though they would do no permanent damage. The experimenter further pointed out that the subject was free to refuse to participate any further without prejudicing herself in any way. For subjects in the no-shock condition, the experimenter spent an equivalent interval repeating the instructions regarding the paired-associate learning procedure.

All subjects were then given four trials of the initial list, the order of the pairs being randomized in three different sequences. Shock subjects received four 1-second shocks, each shock being given 5 seconds after the last S-R pair of each trial was presented. No-shock subjects spent the 12-second intertrial interval on a vowel cancellation task. At the end of the fourth intertrial interval the experimenter pretended that the memory drum was giving trouble, stopped the drum "in exasperation," and said: "Let's stop here. I'll have to see about this machine. . . ." He administered a questionnaire (Wrightsman, 1960), which asked the subject to indicate, on a 6-point scale, her feelings about being shocked (if she was in the shock condition) or about taking part in the experiment (in the case of the no-shock subjects). The scales were defined by the end points "extremely uneasy" (Scale Point 6) and "completely calm" (Scale Point 1). This ended the first learning session.

The experimenter invited all subjects to rest for a while before starting the second learning session. Each subject was directed to a chair (visible from the observation room) while the experimenter checked to see how the other subjects were doing. He stated:

> They should be almost finished with their first learning session. If they are, I think I'll have them wait in here also; we're very short of space in this department, you know, and this is a comfortable enough room, don't you think?"

The experimenter added that to prevent distraction they were not to talk to the other subjects "if they come in here to wait." After 1 minute, subjects in the affiliation condition were joined by two confederates, and 45 seconds after this the third confederate was ushered into the room. All confederates, who wore "dummy" heart-rate electrodes and transmitters, were also instructed, in the presence of the subject, not to talk. The experimenter went out of the room on the pretext of checking with the technicians about the memory drum, and an 8-minute affiliation period ensued. During this interval the experimenter observed the subject from the observation room.

At the end of this period, the experimenter re-entered the experimental room and asked the subject (or subjects), in a matter-of-fact manner, to fill out the postaffiliation anxiety questionnaire "while I check this machine." The experimenter put on the final paired-associate list and fiddled with the memory drum, the whole operation taking about 1 minute. He then collected all questionnaires, sent the confederates back to their "respective rooms." and asked the subject to return to the cubicle. With a few necessary modifications, this procedure was followed with subjects in the nonaffiliation condition.

In order to avoid replacing the shock electrodes on the fingers of the shock subjects when the second learning session began, and thus possibly changing their existing level of anxiety, the experimenter stated to these subjects:

> I'm still a little concerned that this memory drum is not working properly; let's start this session while I keep an eye on the machine to see if it

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[3] Copies of the initial and final lists may be obtained from the senior author.

> works properly. When I'm sure that it is, *I'll set things up as in the first learning session.*

The final list was then presented. At the end of the fourth trial the experimenter indicated that the experiment was over, explained the experiment briefly, and then questioned the subject closely about the different phases of the experiment (e.g., the appearance—whether anxious or nonanxious—of the confederates; the subject's expectation of shock in the postaffiliation phase). From the subjects' responses there is every reason to believe that the manipulations were successful. The shock subjects indicated that they felt no residual pain or discomfort, and all subjects agreed not to reveal the nature of the experiment.

### *Measures*

The experimental session was divided into three main phases: the preaffiliation learning trials, the affiliation/nonaffiliation phase, and the postaffiliation learning trials. Three sources of data are relevant to a test of the hypotheses: behavior, self-report, and physiological measures. All three kinds of data were obtained during the preaffiliation learning trials; self-report and physiological measures were obtained during the "waiting" phase; and behavioral and physiological measures, during the postaffiliation learning trials. A description of these measures follows:

*Behavioral.* Performance of the subjects was the percentage of correct responses given by each subject in the last three trials of both learning sessions; in the case of the final (postaffiliation) learning trials, the percentages of responses to the simple and complex pairs were computed separately.

*Self-report data.* The previously mentioned anxiety scales (Wrightsman, 1960) were employed in assessing the reations of the subjects to the experiment, including, for those in the shock conditions, their reactions to the shock administered. Similar scales were administered to subjects following the affiliation or nonaffiliation phase of the experiment. Two indexes were derived from the postaffiliation anxiety scale scores: (*a*) an absolute index of rated anxiety following the affiliation/nonaffiliation manipulation, in which only the actual postaffiliation anxiety scores were used. This method is similar to that used for securing the preaffiliation anxiety scores; (*b*) an index of anxiety reduction, which was the difference between the postaffiliation and preaffilation scores; this difference was negative (−) if there was a decrease in rated anxiety; positive (+) if the final score was greater.

*Physiological data.* The heart-rate data are described in terms of increase or decrease from a base-line period. That is, the total experimental session was divided into phases demarcated by the critical operations that were recorded by an event marker (see Figure 1). One of these periods was the last 50 seconds of the base-line period (at the beginning of the experimental session). The average of the five highest heart-rate beats (BPM) within each period, including the base line, was calculated for each subject (see Mandler, Mandler, Kremen, & Sholiton, 1961). The base-line rate was then subtracted from the score within a given critical period to provide an "adjusted" measure of emotional response.

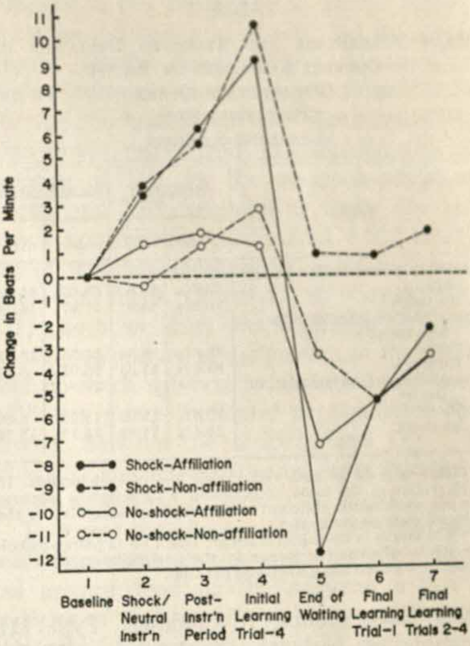

FIG. 1. Heart-rate measures over the seven critical periods. (Based on average of the five highest heart-rate beats in each period, adjusted for base line.)

## RESULTS

### *Behavioral Data*

Table 1 shows the mean percentage of correct responses given on the initial (simple, high-association) paired-associate trials by the four experimental groups, as well as on the simple, high-association pairs of the postaffiliation trials. These data do not support the expectations of Hypothesis 1 that shock subjects perform better than no-shock subjects on a simple paired-associate task. A $t$ test of the differences between the preaffiliation scores of the shock and non-shock subjects was not significant ($t = .24$, $df = 58$). Similarly, these data fail to support the prediction of Hypothesis 2 that the performance, on a simple task, of subjects whose anxiety level was not experimentally reduced by affiliation is superior to the performance of the

TABLE 1

MEAN PERCENTAGE AND STANDARD DEVIATION OF CORRECT RESPONSES ON INITIAL (PREAFFILIATION) AND POSTAFFILIATION LEARNING SESSIONS

| Trials | Affiliation | | Nonaffiliation | |
|---|---|---|---|---|
| | *M* | *SD* | *M* | *SD* |
| Initial (high-association items)[a] | | | | |
| Shock | $74.80_b$ | 13.68 | $68.67_b$ | 16.08 |
| No shock | $70.60_b$ | 14.61 | $74.67_b$ | 14.34 |
| Postaffiliation (high-association items)[a] | | | | |
| Shock | $88.67_b$ | 6.84 | $84.73_b$ | 13.62 |
| No shock | $85.07_b$ | 13.33 | $86.93_b$ | 8.51 |
| Postaffiliation (low-association items[a] | | | | |
| Shock | $30.67_b$ | 15.02 | 15.27 | 8.69 |
| No shock | $26.67_b$ | 11.64 | $25.27_b$ | 15.70 |

Note.—$n = 15$ in each cell. Trials 2–4 of each session. In each section of the table, cells having a subscript in common are not significantly different at the .05 level according to the Duncan multiple-range test.

[a] The means in the initial trials are based on 12 paired-associate items, whereas the means in the postaffiliation trials are based on 6 paired-associate items per trial.

other three groups. A Lindquist Type III analysis of variance of the postaffiliation scores on the high-association pairs revealed that neither the shock nor the affiliation main effect was statistically significant, nor was any interaction. There was, however, a significant main effect for trials ($F = 12.88$, $df = 2/112$, $p < .0001$).

In contrast, the mean percentage of correct responses on the complex, low-association pairs in the postaffiliation trials for the four experimental groups, also shown in Table 1, provided support for Hypothesis 2 regarding the superior performance of nonanxious subjects on a complex paired-associate task. Significant main effects for affiliation ($F = 6.22$, $df = 1/56$, $p < .02$) and for trials ($F = 11.08$, $df = 2/112$, $p < .0001$) were found, as well as a significant interaction involving the shock and affiliation manipulations ($F = 4.29$, $df = 1/56$, $p < .04$). The main effect for shock was not significant.

### *Self-Report Data*

The predicted higher level of anxiety for subjects in the shock conditions received support from the self-report data. A *t* test comparison was made of the self-reported uneasiness of the combined shock and combined no-shock groups in the preaffiliation phase of the experiment. The mean self-reported anxiety score was 4.33 for the subjects under the shock condition and 2.63 for the subjects under the no-shock condition ($t = 4.86$, $df = 58$, $p < .005$, one-tailed).

The mean absolute self-report scores of the four experimental groups in the postaffiliation phase were: shock-affiliation, 2.73; shock–nonaffiliation, 2.87; no-shock–affiliation, 2.06; no-shock–nonaffiliation, 2. These means are not significantly different from one another at the .05 level by Duncan's multiple-range test (Edwards, 1960); however, a $2 \times 2$ analysis of variance of the scores indicated a significant main effect for shock ($F = 5.65$; $df = 1/56$, $p < .025$), but neither the main effect for affiliation nor the Shock × Affiliation interaction was significant.

Table 2 gives the group mean anxiety-reduction scores based on the difference between the preaffiliation and postaffiliation self-reports. The analysis of variance in this instance revealed significant main effects for affiliation ($F = 4.95$, $df = 1/56$, $p < .05$) and for shock ($F = 15.19$, $df = 1/56$, $p < .0005$), as well as a marginally significant interaction involving the shock and affiliation manipulations ($F = 2.81$, $df = 1/56$, $p < .10$).

While the data indicate that the shock-nonaffiliation group experienced less anxiety reduction than the shock-affiliation group, examination of the preaffiliation and postaffiliation scores of these shock groups revealed that the mean postaffiliation scores did not differ in absolute level (2.73 for shock-affiliation; 2.87 for shock-nonaffiliation). The possibility that these two groups might have

TABLE 2

GROUP MEANS AND STANDARD DEVIATIONS OF ANXIETY-REDUCTION SCORES

| Condition | Affiliation | | Nonaffiliation | |
|---|---|---|---|---|
| | *M* | *SD* | *M* | *SD* |
| Shock | −2.00 | 1.07 | $-1.07^a$ | 1.03 |
| No shock | $-0.67^a$ | 0.82 | $-0.53^a$ | 0.74 |

Note.—$n = 15$ in each cell. Prewaiting minus postwaiting scores. Anxiety reduction was assessed by subtracting the postwaiting score from the prewaiting score. Negative values indicate a reduction in anxiety; the greater the negative value, the greater is the anxiety reduction.

[a] Cells having a superscript in common are not significantly different at the .05 level by Duncan multiple-range test.

been significantly different prior to the affiliation treatment, indicating a sampling or procedural error, was investigated by a one-way analysis of variance of the preaffiliation scores for all four groups. This analysis indicated a significant groups effect ($F = 9.56$, $df = 3/56$, $p < .0005$), but a Duncan multiple-range test revealed that the two shock groups were not significantly different at the .05 level of significance. To further determine whether the differences in the anxiety-reduction scores of these shock groups might be attributable merely to the preaffiliation anxiety scores, a procedure was adopted of eliminating certain subjects from each shock group to obtain groups comparable on the preaffiliation measure. Data for 5 subjects in each group, whose scores on the preaffiliation measure could not be matched with an equal score from a subject in the other groups, were eliminated. A $t$ test performed on the anxiety-reduction scores of the remaining 10 subjects in each group indicated that there was a significant difference in the anxiety-reduction scores of the two groups ($t = 2.22$, $df = 18$, $p < .05$, two-tailed).

### *Physiological Data*[4]

The predicted effects of the experimental manipulations on the anxiety levels of the subjects (Hypotheses 1 and 2) are further revealed in the physiological data. Figure 1 indicates group changes in heart rate, adjusted for base-line level of each individual, over the seven critical periods of the experimental sessions. The physiological effect of the shock instructions was assessed by comparing the heart-rate indexes of all subjects during the 50-second period immediately following the administration of the instructions (Period 3). The average increase in heart-rate beats from the base-line rate was 5.36 for the 26 subjects under the shock conditions and 1.50 for the 26 subjects under the no-shock condition ($t = 1.95$, $df = 50$, $p = .03$, one-tailed).

At the end of the learning period (Period 4), during which shock was administered to subjects in this condition, all groups experienced an increase in heart rate, but the shock groups clearly experienced a considerably higher increase—an average of 9.17 and 10.71 for the shock-affiliation and shock-nonaffiliation groups, respectively, as compared with an average of 1.22 for the no-shock–affiliation group and 2.85 for subjects under the no-shock–nonaffiliation condition. A 2 × 2 analysis of variance indicated a significant main effect for shock ($F = 7.77$, $df = 1/48$, $p < .01$); no other effect was significant. A $t$ test of the randomness of assignment of the shock and no-shock subjects to affiliation and nonaffiliation groups indicated no significant difference between the shock groups ($t = .38$, $df = 48$) nor between the no-shock groups ($t = .40$, $df = 48$).

To assess the effects of the affiliation/nonaffiliation manipulation upon the physiological arousal level of the subjects, a 2 × 2 analysis of variance of the heart-rate data at the end of the waiting period (Period 5) was performed; it yielded a highly significant main effect for affiliation ($F = 7.45$, $df = 1/48$, $p < .01$), a Shock × Affiliation interaction that approached significance ($F = 2.23$, $df = 1/48$, $p < .10$), and a nonsignificant main effect for shock. A Duncan multiple-range test indicated that the shock-affiliation group was significantly different from the shock-nonaffiliation group; the no-shock groups, however, were not significantly different from each other, and neither the shock-affiliation group nor the shock-nonaffiliation group was significantly different from the no-shock groups.

During Trial 1 of the final (postaffiliation) learning session (Period 6), the heart rate of the shock-nonaffiliation group (.85) was higher than the other three groups, which were practically identical to one another (−5.48, −5.49, and −5.40 for the shock-affiliation, no-shock–affiliation, and no-shock–nonaffiliation groups, respectively). A 2 × 2 analysis of variance indicated no significant effects, and a Duncan multiple-range test indicated only that on this first trial of the postaffiliation task the difference between the shock-nonaffiliation and the other three groups met at least a .10 criterion of significance.

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[4] The physiological data are based on 13 subjects per cell; the attrition was due to the loss of one heart-rate record from the shock-affiliation group and inadequate records from 7 other subjects.

This pattern of heart rates was maintained during Period 7, which is based on the average of Final Learning Trials 2–4. The heart rate of the shock-nonaffiliation group during this period was again higher than that of the shock-affiliation group; the heart rates of the no-shock groups are again identical and are slightly below that of the shock-affiliation group. However, no significant effects were revealed in an analysis of these heart rates, nor did a Duncan multiple-range test reveal any significant differences between experimental groups at this period.

## DISCUSSION

The behavioral data provided no evidence for the first hypothesis concerning the facilitating effects of anxiety, conceptualized as drive, on the learning of a high-association paired-associate list (Spence et al., 1956). Although the present results are inconsistent with those of Spence, they are consistent with the findings of Korchin and Levin (1957) and of Lee (1961); a difference in methodology, however, may account for the discrepant findings. One interpretation of the present findings of no significant differences in the performance of the groups is that anxious and nonanxious subjects alike found the simple paired-associate items to be very highly associated; consequently, the items were relatively easily recalled by them. This interpretation is currently being tested by "building in" high- and low-associative values in a paired-associate list through differential amounts of exposure to pairs of items, similar to the procedure of Zajonc and Sales (1966).

That the anxiety manipulations attempted in the preaffiliation phase of the experiment were effective was, however, clearly confirmed by the reported subjective impressions of the subjects and by the physiological data. Shock subjects reported significantly more uneasiness and exhibited higher heart rates following the shock instructions and during the initial learning session than their no-shock counterparts.

The performance of the experimental subjects on the high-association pairs of the postaffiliation trials (Hypothesis 2) paralleled their performance in the preaffiliation trials. There were no significant differences among the experimental groups, although the performance of all groups improved over their performance during the preaffiliation trials (86% correct responses, as compared with 72% correct on the initial trials).

However disconfirming may be the findings regarding simple paired-associate items, the effect of anxiety on performance is decidedly more apparent when the task is complex. On the postaffiliation, complex items, the performance of those shock subjects who affiliated was markedly superior to that of the shock subjects who were not allowed to affiliate, as predicted by Hypothesis 2. Further, the mean scores of the no-shock groups were, again as predicted, higher than that of the shock-nonaffiliation group.

The differential effect of anxiety on performance of complex tasks, as compared with simple tasks, appears to be explained most reasonably by the Mandler-Sarason (1952) interference hypothesis, as interpreted by Child (1954). According to Child, task-irrelevant (anxiety) responses are always present in anxious subjects, wheher the task is simple or complex; however, these task-irrelevant responses disrupt performance only in complex situations,

> where the subject is already in conflict between various response tendencies relevant to the task so that the presence of irrelevant response tendencies heightens the conflict and interferes with performance to a greater extent than increased drive improves it [p. 154].

Child's interpretation is also consistent with the aforementioned interpretation regarding the subjects' performance on the simple paired-associate task.

The self-reported impressions of the subjects supported the expectation that anxiety would be reduced through affiliation, at least in firstborn subjects. Although all groups, affiliation and nonaffiliation alike, underwent some anxiety reduction with time—an observation that was also made, but not elaborated on, by Schachter (1959, p. 109)—the amount of anxiety reduction in the case of the shock-affiliation group exceeded that of any other group.

The occurrence of some anxiety reduction in all of the groups prompts a reexamination of the role of distraction in anxiety reduction.

Schachter's hypothesis that affiliation leads to direct anxiety reduction, and Wrightsman's subsequent confirmation of it, has resulted in rejection of distraction, which Schachter calls "indirect anxiety reduction," as a competing alternative (see Schachter, 1959, p. 40). The present findings, however, suggest that distraction, such as thinking of other things than one's anxiety (e.g., personal affairs, other people, school work, etc.) may account, in part, at least, for some of the anxiety reduction observed in the groups. Distraction as a motive for affiliation has been noted among anxious, affiliated subjects (Sarnoff & Zimbardo, 1961), as well as among anxious, nonaffiliated subjects (Bandura & Rosenthal, 1966).

At the same time, the marked anxiety reduction in the shock-affiliation group suggests a more compelling interpretation of the data, namely, that subjects in this group experienced considerable direct anxiety reduction (in Schachter's sense of the term) presumably because the calm, relaxed confederates in the affiliation condition may have functioned as social models. There is considerable evidence that subjects are more influenced by the behavior of others if they are in an aroused state than if they are not; further, that changes in the behavior of an anxious observer are often in the direction specified by the cues provided by a model (Bandura & Walters, 1963, p. 88; McNulty & Walters, 1962; Walters, Marshall, & Shooter, 1960). It is possible to test this latter interpretation by replicating the present experiment with no-shock subjects who affiliate with "anxious" confederates. On the basis of the present theorizing, one would expect no anxiety reduction among such subjects, since they would be presented with a stimulus situation that is anxiety arousing or at least anxiety maintaining, rather than anxiety reducing.

The physiological data, generally speaking, also supported the assumptions concerning the influence of the experimental manipulation of affiliation on anxiety level. The shock-affiliation subjects exhibited a greater decrease in heart-rate level at the end of the waiting period than the shock-nonaffiliation subjects and subjects in the two no-shock groups. The greater decrease in heart rate of the shock-affiliation subjects, in comparison with that of the shock-nonaffiliation subjects, suggests that the influence of the calm confederates may have produced this effect.

The physiological data for the first trial of the final learning session (see Figure 1) indicated that the heart rate of the shock-nonaffiliation group differed from that of the other three groups at a .10 level of confidence. Moreover, the performance of the shock-nonaffiliation group on the complex, low-association pairs of the behavioral task differed significantly from that of the other three groups, supporting the interpretation that differences in performance are attributable to differences in anxiety level.

Thus, the data tend to support the conclusion that anxiety, as induced by the experimental operations of this study, influences performance on a complex learning task in a manner similar to anxiety as assessed by the Taylor *MA* scale. Perhaps more important is the suggestion from the results that it is possible to select behavioral measures that reflect changes in anxiety levels that are usually inferred from self-report or physiological data. These findings raise a question for future research, as to whether a manipulation that reduces situationally aroused anxiety can be employed to reduce chronic anxiety (*cf.* Spence's, 1956, interpretation of chronic and acute anxiety). If so, then high *MA* scale subjects should show improvement in learning a complex verbal learning task following affiliation.

Behavioral measures of anxiety and anxiety reduction (e.g., verbal learning), although a direct measure of the phenomenon, tend to be ignored in social-psychological studies; yet, in the present study, such a measure has been shown to be a relatively effective discriminator of anxiety levels, particularly when complex verbal learning tasks are employed. In contrast, self-report measures may be subject to response bias (Walters & Parke, 1964). It is known, for example, that social desirability does influence subjects' report of their subjective impressions. Physiological measures, also, are problematic; researchers who use these measures are still beset by technological and interpretive problems, the latter growing out of ignorance of the complexity of the physiological processes and their interaction, and of the effects of individual differences.

As more social-psychological research utilizing behavioral measures is undertaken, greater reliance on this dependent variable is a distinct possibility. Given our present state of knowledge of behavioral, self-report, and physiological measures, however, it may be premature to conclude that, by itself, any one of them is a more valid indicator of anxiety than the others. A more realistic position, at the present time, would be that, as suggested by Martin (1961), the use of a *combination* of the three indexes does provide a more comprehensive measurement of anxiety and anxiety reduction. Evidence from the present study suggests that such a procedure can assist more in the interpretation of, and confidence in, the results of studies of anxiety than the single index (self-report or physiological) that is customarily employed in current social-psychological experiments.

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# THE CARRYOVER PHENOMENON:

## THE PERSISTENCE OF REINFORCED BEHAVIOR DESPITE THE ABSENCE OF A CONSCIOUS BEHAVIORAL INTENTION[1]

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Subjects were reinforced for using singular or plural subject words in a sentence-construction task. When this task was mastered, they were informed that they would have to construct a *different* type of sentence in order to receive reinforcement. No solution was possible, however, for reinforcements were then delivered randomly. The results indicate that the original response rule persisted into the early trials of the test series. This effect appeared both among subjects who reported consciously retesting the old hypothesis (despite the experimenter's instructions), and among those who denied that they had returned to the old rule. These results are interpreted as evidence supporting one form of behavior without awareness and are discussed in relation to similar experimental effects reported previously.

In 1955 Greenspoon reported an experiment in which subjects were instructed to say whatever words occurred to them, without putting them into phrases or sentences. The experimenter subtly attempted to reinforce *plural noun* responses by saying "mmm-hmmm" whenever a plural occurred. This led to an increased usage of plural nouns. In a post-experimental interview, relatively few subjects recognized the relationship between their performance and the experimenter's reinforcing behavior; moreover, subjects who were unaware of the experimenter's systematic intervention showed significant evidence of conditioning, suggesting that the effectiveness of verbal rewards did not depend on the subject's insight into the situation. This report of learning without awareness and others like it was subsequently challenged and became the focus of considerable controversy. While more recent studies in this domain have introduced a variety of changes into Greenspoon's experimental paradigm, they have invariably assessed awareness by means of an interview, administered at the end of the experimental session.

In reviewing this literature, many critics have pointed to the inadequacies associated with an end-of-session interview as a means of determining a subject's level of awareness during the *period of acquisition*. One difficulty revolves about the problem of *suggestion*, for it is possible that the apparent awareness shown by some subjects was stimulated by the interviewer's questions, and thus could not account for responses that had previously been emitted. A related criticism is based on the possibility that a subject's awareness may sometimes result from self-observations *after* conditioning has taken place, when he first notices the systematic patterning of his responses. These criticisms suggest that some subjects may mistakenly be classified as "aware," although they are, in fact, *unaware* of the response-reinforcement contingency during the trials that the experimenter wishes to account for. Misclassification of subjects who are (or were) "truly" aware may also occur, for it is possible that the subject has forgotten the "rule" that he followed earlier in the experimental session; this is particularly likely when an extinction series has been interposed between the acquisition trials and the interview.

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The use of verbalization as an index of awareness presents yet another problem, since misclassifications may be brought about by *correlated hypotheses* (Adams, 1957). That is, a subject may fail to discover the exact reinforcement rule specified by the experimenter, but may, nevertheless, verbalize a *related* performance strategy, which if followed consistently will lead to responses that are generally (but not always) correct. For example, Dulany (1961, 1962) has shown that in a continuous-association task like the one used by Greenspoon, subjects may develop the hypothesis that when they are reinforced, they are to continue generating words in the "same semantic category." When this strategy is followed, and the experimenter reinforces plural nouns, for example, "shoe, house, cars [mmm-hmmm], boats, [mmm-hmmm], ships, [mmm-hmmm] . . ." the resulting associations will show an increasing frequency of plurals, for the set to give a semantic associate normally yields responses which agree in number with the preceding word in the chain; hence if the previously reinforced word was a plural and the subject searches for a semantic associate, his verbal habits usually lead him to respond with another plural.

The present experiments were designed to investigate one form of behavior without awareness using a methodology which would hopefully eliminate many of the weaknesses inherent in the postexperimental interview. The basic plan was to reinforce a selected response pattern in a sentence-construction task until complete mastery had been achieved. The subject was then instructed that having solved this first problem he would have to construct a *different* type of sentence in order to be reinforced; the instructions emphasized the fact that this new solution was *unrelated* to the rule that had "worked" in the first task. Unknown to the subject, however, there was *no* new solution, for reinforcements were now delivered randomly. The sentences generated in this last series were examined to see if they revealed any carryover from the previously reinforced pattern. A significant carryover from the first task would be interpreted as behavior without awareness if the subject had followed the experimenter's instructions, by consciously searching for a totally new solution, orthogonal to the rule that had previously been reinforced. Such data would indicate that overt actions may follow a systematic pattern which is inadvertently consistent with the subject's previous reinforcement history, even if this pattern is unrelated to the individual's conscious plans at the time the behavior is enacted.

## EXPERIMENT I

### *Method*

*Overview.* Subjects first participated in a sentence-construction task in which some were reinforced for sentences that involved *singular* subjects, while others were reinforced for *plural* subjects. In order to explore the effects of overlearning, subjects in differing subgroups were required to complete 10, 20, or 30 consecutive errorless learning trials before the presentation of (*a*) 10 "bridging" trials (see following), and then (*b*) the terminal test series, designed to assess the carryover phenomenon.

The basic design thus involved six subgroups, which resulted from the application of two reinforcement rules (singular versus plural) and three levels of overlearning (10, 20, or 30 errorless trials) on the first series of sentences. Eight paid subjects, undergraduates at the University of Michigan, were assigned to each group, with the individual assignments being determined by the order in which the students appeared. (The first subject was placed in the 10-trial–singular condition, the second in the 10-trial–plural group, and so on.) If a given subject did not start his criterion run before Trial 60, he was discarded from the sample and the next subject to appear was assigned his place in the series. Out of 67 subjects who were recruited, 19 failed to master the first task and 48 succeeded.

*Warm-up.* To reduce the number of subjects who might otherwise fail to learn List I (see following section), the experimental session started with a brief concept-formation task. Subjects were shown 15 4 × 6-inch cards, one at a time; on each card there were two geometric figures which varied in several ways. The subject was instructed to choose one figure on each card, following which the experimenter would indicate whether the choice was right or wrong. The subject was to continue doing this until he could formulate the rule that was guiding the experimenter's reactions. In order to be consistently correct, the subject had to choose the smaller of the two figures. All subjects continued in this task until they correctly identified the underlying rule; hints were provided, when necessary, to insure successful solution. Pilot work had indicated that this warm-up task facilitated performance on the subsequent sentence-construction task, presumably because both tasks involved the selection of a specific dichotomous dimension out of a larger set. Following these concept-formation trials, the experimenter explained that the next experimental task was similar to the warm-up, but noted that it involved "pairs of words instead of figures."

*List I.* On each trial, subjects were presented with a pair of nouns, one singular and one plural; the nouns were exposed on a Stowe memory drum. On each trial the subject was to make up a sentence which was to begin with the word "the," followed by one of the two nouns, as the subject of the sentence (e.g., the *boy* liked his drum). The experimenter explained that the subject's task was to get him (the experimenter) to say "right" as often as possible. He indicated that the rule was the same on every trial and did not involve complicated sequences or combinations of rules. After the subject had completed four practice sentences to familiarize him with the task, the experimenter (who was separated from the subject by a plywood screen) announced whether each subsequent response was "right" or "wrong."

*Bridging trials.* When a criterion performance of 10, 20, or 30 consecutive errorless trials had been achieved on this first task (depending on the subject's group assignment), the *character* of the stimuli was changed, but subjects were nonetheless instructed to follow the same rule as before. The instructions read:

> All right, from now on, what you will see in the window of the drum will be somewhat different, and you will correspondingly have to respond in a different way. Now, instead of two *words,* you will see two *letters.* We again want you to make up a sentence, but this time, we want you to use one of the letters as the first letter in a noun which will be the second word in your sentence, after the word "the." That is, say the letters are "W" and "A." You might say something like, "The *woman* wore yellow shoes," or else you might say, "The *army* retreated quickly." Any questions? Now you probably had the right rule in mind for the last list, because you got the last 10 [or 20, or 30] in a row right. What we want you to do now is to continue to use that same rule in the formation of these new sentences, because the same rule is correct for both types of lists.

A series of 10 trials followed, with the experimenter reinforcing all correct responses. The few errors that occurred were invariably due to a misunderstanding of the experimenter's instructions; in such cases, the experimenter clarified the instructions without explicitly stating the previous reinforcement rules. These trials were intended to provide a bridge between the initial learning task and the carryover trials which were to follow, where letter pairs were again used as stimuli. It was hoped that this brief practice in applying the singular (or plural) rule in conjunction with the letter stimuli would facilitate a carryover effect on subsequent trials, when similar pairs of letters were to be presented in an ostensibly new problem.

*Carryover test.* After the 10 bridging trials, the experimenter read the following instruction:

> Now I'm going to continue to give you sets of letters, and we want you to continue making up sentences, but in this next part, *the rule that you've been using up to now has nothing to do with whether the sentence is correct or not.* What we'd like you to do now is to find a *new rule* for making up correct sentences. Sometimes a correct sentence might be in accord with the old rule, sometimes it might not. *The old rule is no longer relevant one way or the other.*
>
> Also, we'd like you to make up your sentences in a slightly different way. We'd like you to continue using the same general format, that is, starting the sentence with "the," and using a noun as the second word (and subject) of the sentence, but now we'd like you to use one of the two letters as the first letter in the *last* word of the sentence, rather than as the first letter of the *second* word. Do you understand? One more thing, we'd like you to avoid as much as possible using the same word over and over again as either the subject or verb of your sentence.

The instruction to use the letter stimuli in choosing the *last* word of the sentences to be constructed was intended to focus the subject's attention on potential solutions which would be largely orthogonal to the singular versus plural status of the initial noun (following the starting word "the"). In this way subjects were encouraged to sample and test hypotheses which would not *determine* the subject-noun they selected, so that the carryover effect might operate reasonably automatically, freed from the constraints that would normally be imposed by deliberate cognitive choice.

A total of 60 carryover trials were conducted. Following each response, the experimenter responded "right" or "wrong" according to a fixed random schedule, which was constructed so that within each block of 10 trials the "correct" response required a singular subject-noun on 5 trials and a plural noun on the remaining 5.

*Interview.* When the carryover series was completed, the experimenter checked on the success of his manipulations by asking each subject if, in attempting to solve the last task, he had ever returned to using the "old rule," the one that had worked previously. While each subject had been explicitly instructed that the last series required a new solution, it still seemed desirable to see if he had at any time consciously returned to the "old rule" in the search for a solution.

### *Results*

In analyzing the carryover series the 60 test trials were divided into six 10-trial blocks. Each sentence (response) was classified into one of two categories, depending upon the number (singular versus plural) of the initial noun, following the beginning word, "the." Since there were only two response categories, the increased usage of one response class (e.g., plural) simultaneously reflected a decreased usage of the other (singular). Each subject's performance could thus be fully described by simply counting the number of plural responses emitted within each trial block; a subject (or group) that produced many plural responses might alternatively be described as producing relatively few singulars.

Figure 1 presents the mean number of plural responses given by subjects who had been assigned to the singular and plural reinforcement conditions, after the various overlearning conditions had been combined (this variable proved to be insignificant, see following). Note that on five of the six test blocks the subjects who had been reinforced for plurals generated more plural responses than did subjects reinforced for singulars; it is, however, apparent that this group difference diminished over trials. This pattern of results necessitates a parallel conclusion regarding the usage of singular responses. That is, if the group reinforced for singulars generated fewer plural responses in five of the six test blocks, they must have produced more *singular responses* within these same five blocks (since each response was classed as either a singular or a plural).

The data obtained in Experiment I were analyzed by means of a three-way analysis of variance, with repeated measurements on one variable (Winer, 1962). The results of this analysis are presented in Table 1, which reveals a significant interaction ($p < .01$) between the six trial blocks and the subject's assignment to the singular versus plural reinforcement condition. Inspection of Figure 1 suggests that this interaction mainly reflects an initial group difference which was subsequently dissipated, so that the main effect associated with a subject's prior reinforcement history did not quite reach conventional levels of significance. The unexpectedly significant effect associated with trial blocks primarily reflects a gradual reduction in plural responses by subjects assigned to the plural reinforcement condition (from an initial high of 3.1 to a low of .9 on Block 6); this decline, in combination with the constant performance of the "singular" subjects (at a rate of approximately 1 plural response and 9 singulars per block of 10 trials), produced a gradual diminution in plurals for all subjects taken together. Note, finally, that the number of trials to criterion on the first task had no significant impact on the carryover series, nor was it associated with any significant interactions.

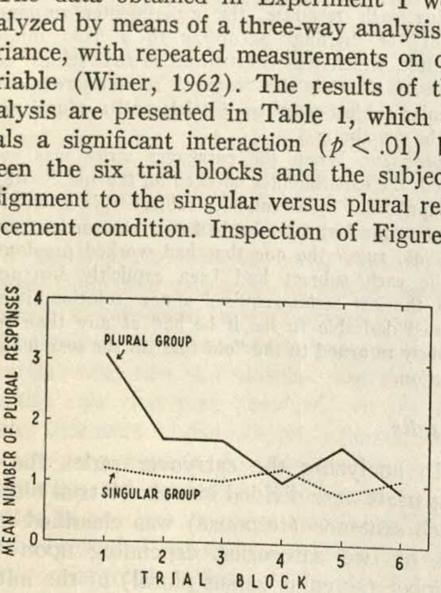

FIG. 1. Number of plural responses given by subjects who had previously been reinforced for singulars and plurals. (Each trial block represents 10 trials.)

TABLE 1

ANALYSIS OF VARIANCE FOR PLURAL RESPONSES: EXPERIMENT I

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Between *S*s | | | |
| Singular-plural (A) | 1 | 25.09 | 3.48* |
| Trials to criterion (B) | 2 | 6.10 | .84 |
| A × B | 2 | 16.26 | 2.25 |
| Error (b) | 42 | 7.22 | |
| Within *S*s | | | |
| Trial blocks (C) | 5 | 8.66 | 4.33*** |
| A × C | 5 | 6.64 | 3.32** |
| B × C | 10 | 1.48 | .74 |
| A × B × C | 10 | 1.10 | .55 |
| Error (w) | 210 | 2.00 | |

* $p < .10$.
** $p < .01$.
*** $p < .001$.

*Awareness.* When questioned at the end of the experimental session, 18 of the 48 subjects indicated that despite the experimenter's instructions, they had at some point attempted to apply the previously reinforced rule in searching for a solution to the carryover series; these subjects were classified as *aware.* Fortuitously, half of the subjects in the aware group and half of those in the unaware group had been initially reinforced for singular responses, while the remaining subjects had been reinforced for plurals. Inspection of the data revealed that for the test series as a whole, the carryover effect was somewhat more pronounced among the *unaware* subjects (those who had *not* consciously returned to the previously reinforced hypothesis), rather than among those who were aware. Figure 2 graphically depicts the carryover effect for the aware and the unaware groups; these curves were developed by calculating the average difference in plural responses between "singular" and "plural" subjects who were aware, and those who were not aware. As in Figure 1, this graph combines data from subjects who had been exposed to different overlearning conditions prior to the carryover series.

Unfortunately, inclusion of the awareness factor as an additional variable led to some very small subgroups, with *n*'s as small as 2. In order to assess the carryover results separately for aware and unaware subjects who had been exposed to each of the three overlearning conditions, the analysis of variance approach was therefore abandoned, and the nonparametric *U* test (Siegel, 1956) was applied. Each test compared a selected subgroup of singular and plural subjects. Two dependent variables were used: (*a*) the number of plural responses in the first 10 test trials, and (*b*) the number of plural responses over the entire set of 60 test trials. In each case the authors wished to test the hypothesis that the subjects who were previously reinforced for plurals had emitted more plural responses (and hence fewer singulars) than those who had been reinforced for singulars. Table 2 presents the *p* values obtained for the various *U* tests.

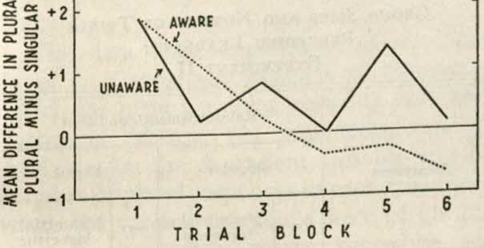

FIG. 2. Mean difference between singular and plural subjects (positive differences provide evidence for the carryover effect). (Data are plotted separately for aware and unaware subjects.)

TABLE 2

*U* TEST RESULTS FOR THE CARRYOVER SERIES

| Task I criterion | *N* | 1st 10 test trials | All 60 test trials |
|---|---|---|---|
| | Unaware *S*s | | |
| 10 trials | 4S,5P | .02 | *ns* |
| 20 trials | 5S,6P | .02 | .01 |
| 30 trials | 6S,4P | *ns* | *ns* |
| All groups | 15S,15P | <.001 | <.05 |
| | Aware *S*s | | |
| 10 trials | 4S,3P | .05 | *ns* |
| 20 trials | 3S,2P | .10 | *ns* |
| 30 trials | 2S,4P | *ns* | *ns*[a] |
| All groups | 9S,9P | <.01 | *ns* |

Note.—All *p* values are one tailed. Abbreviations: S = singular subjects; P = plural subjects.

[a] Of all the comparisons presented in this table, this is the only case in which the mean trend of the data was inconsistent with the anticipated carryover effect.

Although several of the *U* tests failed to achieve an acceptable significance level, it is worth noting that in 15 of these 16 (nonindependent) comparisons the data followed the predicted pattern—subjects who had initially been reinforced for plurals gave more plural responses (and fewer singulars) than subjects who were reinforced for singulars. Inspection of Table 2 indicates that the carryover effect was most clearly manifested within the first 10 test trials, and was "diluted" when the data from all 60 test trials were considered. There is also some suggestion that the unaware subjects produced more clear-cut positive results than those classed as unaware; however, this is partly due to the larger *N*s included in the unaware comparisons. Lastly, by inspection, it appears that the carryover effect was most clearly revealed among subjects who were assigned to the 20-trial criterion group; this was particularly true among the unaware subjects, where the 20-trial condition not only produced a significant carryover on the first 10 test trials, but also led to a significant "main effect" when the entire carryover series was considered.

## EXPERIMENT II

### *Method*

Experiment II was mainly designed as a replication of Experiment I; certain methodological changes were, however, introduced, in an attempt to provide better experimental control.

The experimental procedures were essentially unchanged from Experiment I. However, *all* subjects were now required to reach a criterion of 20 consecutive errorless trials before being switched to the bridging trials (the earlier results had suggested that the 20-trial condition might be the most favorable for the production of subsequent carryover effects, although the differences here were far from significant).

*Apparatus.* Experiment II employed a more automated method for delivering reinforcements. Instead of having the experimenter say "Right" or "Wrong," the plywood barrier which hid the memory drum and separated experimenter and subject was equipped with two small lights, placed just above the aperture through which the stimuli were exposed. One of the lights was green, with a check mark printed on it (for correct responses), while the other was red, and was marked with an "X" (for incorrect). The experimenter controlled the onset of these lights by means of a toggle switch. This modification provided assurance that the reinforcements were uniform in intensity and duration throughout, for it had been feared that otherwise the experimenter might inadvertently reinforce carryover responses relatively *heavily* (when reinforcement was called for by the random schedule), while providing smaller verbal rewards for noncarryovers that were to be reinforced.

A second apparatus change involved the use of a tape recorder to permit an independent rescoring of the test series, to guard against the possibility that the experimenter had inadvertently scored more carryovers than the data warranted. The tape recording also provided an opportunity to check on the reliability of the aware-unaware categorization.

*Interview.* A somewhat more formal posttest interview was used, consisting of the following questions:

1. What types of rules were you trying out on that last list? [This question was asked to determine whether the subject would spontaneously volunteer having gone back at some point to using the old rule, or some variant of it.]
2. Do you remember the rule you used on the first list? What was it?
3. When I told you that the old hypothesis was no longer relevant, what did you take it to mean, in terms of what you were going to do on the rest of the list?
4. Did you ever go back to trying the old rule again? Did you ever try its opposite? At what point in the list? For how long?

*Subjects.* The subjects in this experiment were again undergraduates at the University of Michigan who were paid for their participation. Seventy-four subjects were alternately assigned to the singular and plural reinforcement conditions. Following the elimination of 19 subjects for failure to learn the preliminary list (9 who had been reinforced for singulars and 10 for plurals), and 5 subjects whose status as aware versus unaware was ambiguous (the experimenter and an independent judge who heard the tapes had reached conflicting conclusions), there remained 50 "usable" subjects. Table 3 shows the size of the relevant experimental groups, and the mean number of trials preceding the criterion run for each one.

Although the original experimental plan was based on the assumption that all subjects would be classed as either aware or unaware, a third group, henceforth referred to as *partially aware,* was assembled when it was noted that there were 11 subjects who denied trying the "original" singular-plural hypothesis during the carryover series, but volunteered that they had tried a modified version of this rule; these subjects had tested the "number" hypothesis as a basis for selecting the *last* word in their sentences (the initial task involved the application of this rule in the selection of the *second* word). This raised the possibility that these subjects might have been following a correlated hypothesis. Although an analysis of the carryover series revealed that there was, in fact, no significant correlation between the number (singular versus plural) of the subject word and the number of the last word, data from the partially aware subjects were nevertheless analyzed separately.

## *Results*

Carryover effects were assessed by comparing the number of plural responses emitted by subjects who had experienced contrasting reinforcement histories in the initial sentence construction task. Figure 3 presents the mean differences between those who had been reinforced for singulars and plurals, as they proceeded through successive 10-trial blocks in the test series; positive differences are consistent with the anticipated carryover effect, for they signify that subjects reinforced for plural responses had emitted more plurals (and fewer singulars) than subjects in the singular group. Difference scores are plotted separately for subjects identified as aware, un-

TABLE 3

GROUP SIZES AND NUMBER OF TRIALS PRECEDING LEARNING: EXPERIMENT II

| Awareness | Reinforcement condition | | | |
|---|---|---|---|---|
| | Singular | | Plural | |
| | *n* | Mean trial of last error | *n* | Mean trial of last error |
| Aware *S*s | 7 | 3.6 | 9 | 11.3 |
| Partially aware *S*s | 5 | 13.0 | 6 | 13.2 |
| Unaware *S*s | 13 | 15.2 | 10 | 8.1 |

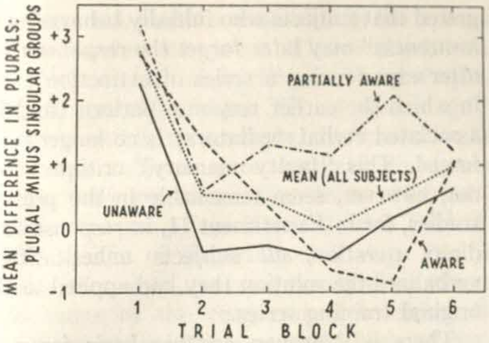

FIG. 3. Mean differences between singular and plural subjects (positive differences provide evidence for the carryover effect). (Data are plotted separately for aware, partially aware, and unaware subjects.)

aware, and partially aware; Figure 3 also shows the mean results for the three groups combined. The graph indicates that in the first block of 10 trials the plural subjects generated from two to three more plural responses than did subjects who had been reinforced for singulars; this pattern was true at all three levels of awareness, and confirms the results of Experiment I. The succeeding trial blocks show carryover effects that are uniformly below this initial level, although the partially aware group remains above the zero line throughout. While plural responses were arbitrarily chosen as the dependent variable in assessing this experiment, the fact that the plural reinforcement condition led to more plurals is an unmistakeable sign that subjects in this group emitted *fewer singulars*. These were, after all, the only two response classes that were possible (each subject-noun was either a singular or a plural); hence a group that produced many plurals could not have produced many singulars.

The data from Experiment II were analyzed in a three-way analysis of variance in which the main variables were (*a*) *prior reinforcement history*, (*b*) *awareness*, and (*c*) *trial blocks;* the dependent variable was the number of plural responses in each trial block. Since the various subgroups were of unequal size, an unweighted-means procedure was adopted (Winer, 1962, pp. 374–378). The summary of this analysis is presented in Table 4, which revealed, most importantly, that there was a significant difference between the performance of subjects in the singular and plural conditions—the plural group emitting more plural responses overall. The fact that there was also an *interaction* between a subject's initial reinforcement history and the successive trial blocks (A × C) indicates, however, that the group differences due to reinforcement were not stable. This can be seen most clearly in Figure 3, which plots the obtained difference in plurals between subjects in the two reinforcement conditions; there is an obvious reduction in the difference between the two groups as they proceeded through the test list, with some mild hint of an upturn (increased group difference) in the final two blocks.

The main effect attributable to trial blocks was also significant (as in Experiment I), and again this was primarily because of the gradual decline in plural responses by subjects who had initially been reinforced for plurals, while those reinforced for singulars exhibited a more stable response pattern. The presence of this last-mentioned trend in both experiments suggests that the reinforcement procedure may have had its greatest impact on the "plural" subjects, by temporarily elevating what would normally have been a rather low frequency of selection for plural subject words. (In both Experiments I and II, a pooling of *all* subjects revealed a mean output of about 1.5 plural responses per block of 10 test trials.) By contrast, the stable performance of the "singular"

TABLE 4

ANALYSIS OF VARIANCE FOR PLURAL RESPONSES: EXPERIMENT II, ALL TRIALS

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Between *S*s | | | |
| Singular-plural (A) | 1 | 50.76 | 5.56* |
| Awareness (B) | 2 | 14.24 | 1.56 |
| A × B | 2 | 9.33 | 1.02 |
| Error (b) | 44 | 9.13 | |
| Within *S*s | | | |
| Trial blocks (C) | 5 | 7.98 | 3.58** |
| A × C | 5 | 10.30 | 4.62*** |
| B × C | 10 | 2.06 | |
| A × B × C | 10 | 1.75 | |
| Error (w) | 220 | 2.23 | |

* $p < .025$.
** $p < .005$.
*** $p < .001$.

TABLE 5

ANALYSIS OF VARIANCE FOR PLURAL RESPONSES: EXPERIMENT II, TRIAL BLOCK I

| Source | *df* | *MS* | *F* |
|---|---|---|---|
| Singular-plural (A) | 1 | 83.23 | 12.19* |
| Awareness (B) | 2 | 1.09 | |
| A × B | 2 | .75 | |
| Within cells | 44 | 6.83 | |

* $p < .005$.

group suggests that for these subjects the earlier reinforcement procedures may have had scant impact, perhaps because the singular response to the test items would have been strongly predominant even *without* experimental intervention, and hence could not be appreciably strengthened during the training series.

In order to focus more precisely on the early portions of the test list, where the carryover effect was most clear-cut, an additional analysis was conducted based solely on the data obtained in the first block of 10 trials. The results are presented in Table 5, which reveals a strong effect associated with the subject's prior reinforcement history ($p < .01$), and *F*s *less than 1* for the main effect and interaction term involving the awareness variable. A final analysis, based solely on data generated by the unaware subjects in the first 10 test trials, also revealed a highly significant difference between those who had been reinforced for singulars, versus those reinforced for plurals ($t = 3.35$, $df = 21$, $p < .01$).[3]

### Discussion

*Awareness.* Although the data from both Experiments I and II were quite consistent in showing significant carryover effects, caution seemed warranted in interpreting the data relating to awareness. The main problem here was the possibility that some of the subjects who had actively retested the "old" hypothesis during the carryover series might nevertheless have been classified as unaware because of weaknesses in the interview procedures. Some critics, for example, have suggested that subjects who initially behave "with awareness" may later forget the response rule, after experiencing a series of extinction trials in which the earlier response pattern (and its associated verbal mediators) is no longer reinforced. This "faulty memory" critique does not, however, seem reasonable in the present studies, for in Experiment II, in response to a direct question, *all* subjects unhesitatingly verbalized the solution they had applied to the original training series.

There is, however, another basis for concern. Perhaps some of the subjects who had consciously reverted to the "old" hypothesis for some of the test trials were chary about admitting it, for reversions of this type were at variance with the experimenter's explicit instruction to discard the old solution and search for a new one. A subject who was concerned about this point might well be less than candid in describing the solutions he had attempted and might mistakenly be classified as unaware. This line of argument casts some doubt on the "purity" of the unaware groups, for it is possible that they included a certain number of subjects who had consciously reverted to the old hypothesis. If this was true, the fact that the unaware subjects showed significant carryover effects in both experiments might simply be due to the behavior of those who were actually aware, but had been misclassified—a skeptic could contend that the "truly unaware" might show no carryover.

Despite the plausibility of this argument, it does not fit the present data, if we consider it in greater detail. The problem is simply this. The critique is based on the two assumptions: (*a*) Carryover effects occur only among subjects who have consciously reverted to the "old hypothesis," and (*b*) the seemingly significant effects obtained with subjects identified as unaware are attributable to misclassifications. If these assumptions are valid, however, we would anticipate stronger carryover effects in the aware group, as compared with the subjects classed as unaware; the basis for this prediction is the plausible belief that if the interview procedure had *any* degree of validity (even granting some misclassifications), the group labelled as unaware must have included a far smaller percentage of subjects who had consciously tried the old response rule, and hence should, on the average, show far less carryover. The results on this point are quite clear, however, and they do not support the above line of reasoning. In Experiment I, for example, the results for the unaware subjects were (surprisingly) somewhat *more* significant than those obtained in the aware group (see Figure 2; also Table 2). The results of Experiment II were also inconsistent with the misclassification theory, for none of the comparisons involving the awareness variable approached significance. Thus, in the overall analysis of variance (see Table 4), the critical interaction of awareness with the originally reinforced response rule (singular or plural) had an $F$ of 1.02, while the triple interaction of these two variables with the successive trial blocks yielded an $F < 1$. Finally, when a separate $2 \times 2$ analysis of variance was carried out with data from just the first 10 test trials, the awareness variable was again far from significant, with the critical interaction of awareness and prior reinforcement showing an $F$ less than 1 (see Table 5). This consistent failure of awareness to influence the carryover effect provides strong evidence against the misclassification hypothesis as an explanation for the present results; this means that we cannot account for the behavior of the unaware group by claiming that many of these subjects were "really" aware, but were misclassified for one reason or another.

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[3] Similar results were obtained when these data were analyzed with the nonparametric *U* test (Siegel, 1956).

A final explanation that was considered was the possibility that the subjects had erroneously responded to the interview by stating only the hypotheses they had tried in selecting the *final* words of the carryover sentences. While the interviewer was mainly interested in knowing whether the subject had consciously returned to the singular versus plural hypothesis in selecting *initial nouns* (the second word of each sentence), a rereading of the questions that formed the core of the interview (see Method) suggested that there might be some likelihood of misunderstanding on this point.[4] This explanation implies that for some subjects there may have been a lack of correspondence between the behavior that was observed (carryover of initial nouns), and the intentions that were verbalized (focusing on the *final* words to be included in the different sentences). The results that were obtained might be mainly due to the performance of these subjects.

To assess this possibility, a research assistant who had not previously participated in this project checked each of the tape-recorded interviews in Experiment II. He found that in 41 out of 49 interviews[5] the subjects had been unambiguously interviewed concerning the possibility that they had reapplied the singular (or plural) hypothesis in selecting initial nouns for the carryover sentences. There were, however, eight cases in which the interview had been *ambiguous* and was not clearly focused on the selection of the initial nouns. Of these 8 ambiguous interviews, 7 involved subjects who had been classed as unaware, and 1 that was classed as partially unaware. This left a total of 16 unaware subjects whose interviews were unambiguous and 10 partially aware subjects with unambiguous interviews. The relatively high frequency of unaware subjects among those whose interviews were judged ambiguous may reflect the frequently unclear and rambling verbalizations of these subjects, which may have led the interviewer to conclude that further and more explicit questioning would have been fruitless.

The next step was to see if the results of Experiment II would be changed if those subjects whose interviews were ambiguous (in the sense noted above) were to be eliminated. Since the main evidence for the carryover phenomenon appeared within the first 10 trials (see Figure 3; also Table 5), this portion of the data was reanalyzed, using only subjects whose interviews permitted an explicit and unambiguous determination regarding their intentions (if any) in selecting the initial nouns. The results were unchanged from those reported in Table 5. Indeed, the $p$ value associated with a subject's prior reinforcement history (singular versus plural) became even more significant, while the results attributable to awareness, and the interaction of awareness and prior reinforcement history, were far from significant.

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[4] On the other hand, the interview almost always involved a certain amount of elaboration and further exploration beyond the specific points listed, until the interviewer was satisfied with the information he had obtained.

[5] The recording of one interview was unavailable.

Another analysis of these first 10 trials was then performed which included only subjects who had been classed as unaware, whether or not the interviews had been judged ambiguous in the sense discussed above. There were four groups of subjects, arranged in a factorial design involving (*a*) prior reinforcement history (singular versus plural), and (*b*) the judged ambiguity of the interview (ambiguous versus nonambiguous). Once again, reinforcement history proved to be highly significant ($p < .005$); the ambiguity variable was far from significant, either as a main effect, or as a contributing variable in the interaction of ambiguity and reinforcement history.

These results provide convincing evidence aaginst the view that the carryover effects of Experiment II were due to subjects who had misinterpreted the focus of the postexperimental interview. Unfortunately, it was impossible to perform this type of analysis on Experiment I, since those interviews were not tape-recorded; in view of the similarity of the two studies, however, it seems unlikely that the outcome there would have been any different.

*Related findings.* The present results are consistent with other experimental data in showing that previously reinforced behavior may be maintained for a time in the absence of any conscious intention to do so. A recent demonstration of this principle was reported in a study by Bridger and Mandel (1965), where the GSR was conditioned by pairing a light flash (CS) and a painful electric shock (UCS). Following an acquisition series of 20 trials, one-half of the subjects were *informed* that no more shocks would be delivered and the shock electrodes were removed. Nevertheless, despite this group's recognition that the shock series was finished (a point that was carefully checked in a postexperimental interview), subsequent presentations of the CS continued to elicit GSRs, leading to the conclusion that this response was not fully controlled by the subjects' conscious expectations. It is of some interest to note, however, that among these *informed* subjects, the reinforcement schedule that was employed in the acquisition series (continuous versus partial) had *no effect* upon extinction, while the classic partial reinforcement effect was obtained when subjects were *not informed* of the transition from acquisition to extinction.

A related carryover phenomena has been reported by Postman and Adams in their studies of incidental learning (1954, 1955). Subjects in those experiments were recruited for what appeared to be an ESP study; they were presented with a series of words and were instructed to guess the number between 1 and 10 that the experimenter had paired with each word. Following each response, the experimenter announced whether the subject's guess was "right" or "wrong." After a single presentation of the word list the entire series was repeated, but no feedback was given; a critical point in these experiments is the fact that subjects were led to believe that a response which was correct on the first trial might or might not be correct when the stimulus word was next presented. Despite this, on the second presentation of the word list subjects repeated previously rewarded responses more often than they repeated responses that had been punished. Thus, reinforcement led to repetition, even though this was not a strategy that would be followed *deliberately*, given the subject's understanding of the situation.

While the present results parallel those of Postman and Adams (1955), these experiments differ in the specificity of the responses that were studied, for Postman and Adams were concerned with repetitions of the *particular* responses that had initially been reinforced, while the present experiments focused on the carryover of more *general* response classes (e.g., sentences with singular or plural subject words). In both series of experiments, however, the carryover effect was demonstrated in connection with a *voluntary response,* indicating that this effect is not restricted to autonomic conditioning, as in the work of Bridger and Mandel (1965).

These experiments point to the individual's inability to voluntarily "erase" the effects of his prior experimental history. That is, although subjects in the present studies were essentially informed that they should forget about previously reinforced response patterns while searching for a new solution, their failure to do this (the persistence of the original response class) points to an inability to simply discount prior experiences. While we might be tempted to conclude that the obtained carryover is an automatic consequence of reinforcement, the present evidence is somewhat equivocal on this point, since it is conceivable that similar effects might have been produced by *instructing* subjects to make up sentences with singular subjects (for example), and then checking for carryover effects after they had been instructed to search for some *different* solution.

Despite the replicability of these results, it is likely that the carryover phenomenon could be reduced, if not overcome completely, by more *specific* instructions which concretely informed the subject as to the exact performance rule that he was to adopt, rather than informing him that he was to search for some "new" (but otherwise unspecified) response pattern. In the present task, for example, a given subject might be told that he was no longer to make up sentences with singular subject words, but was now to construct sentences with *plural* subjects. While we would anticipate virtually immediate compliance with this instruction, it would nevertheless be interesting to see if the carryover effect might still be manifested in the form of longer latencies when the new solution conflicted with the prior reinforcement rule.

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